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ORIGINAL ARTICLES.

Notes on the Anal Plates of *Eupachycrinus calyx* and *Zeacrinus konincki*.

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(PLATES XV-XVII.)

Introduction.

Many years ago, after a short experience in collecting crinoid cups at the localities herein noted, especially that of Invertiel, it became evident to me that there was considerable variation in the disposition and shape of the anal plates in the cups of *Eupachycrinus* and in a lesser degree in those of *Zeacrinus* and other genera. From this I thought it might be possible, in a large number of specimens, to trace the evolutionary stages in the passage of these plates upwards in the cup, and perhaps even their elimination from the limits of the cup. With thoughts such as these in mind, I have in subsequent years laid aside the majority of the cups I have found, and in 1918 (*Trans. Geol. Soc. Glasgow*, vol. xvi, pt. iii, p. 376) gave a short account of an examination of 200 cups of *Eupachycrinus*, 45 of *Zeacrinus*, and 50 of *Hydrieonocrinus*. This examination was necessarily hurried, owing to the circumstances of the time, but it convinced me that some interesting results would be obtained from a larger series. It was not, however, till recently that I was able to begin this work, my original intention being not only to deal with the cups of the three genera already mentioned but also with a considerable number belonging to *Ulocrinus* and *Woodocrinus*. As the sorting and cleaning of the specimens proceeded it became evident that this would occupy considerable time. I have accordingly thought it advisable, in the first instance, to confine attention to *Eupachycrinus* and *Zeacrinus*, and in this communication propose only to give the results of the study so far as it has gone. This relates to two species, namely, 1,000 cups of *Eupachycrinus calyx*, and 342 of *Zeacrinus konincki*—all the specimens belonging to these two species which I had laid aside up to this date (1924). The examination and details of the additional cups of *Hydrieonocrinus* collected since 1918, together with the series of *Ulocrinus* and *Woodocrinus,* and their bearing on the questions raised by the present study, must be deferred to another time.

Of course, in considering statistics of the kind here given, one has always to remember that the figures represent only the gleanings from a few square yards of the Carboniferous sea floor and, comparatively large though the numbers may be as applied to crinoid cups, even so perhaps only indicate in a general way the trend of the evolutionary impulses. As has so aptly been written by Wachsmuth and Springer (*North American Crinoidea Camerata,* vol. i, p. 167): "The trouble is that our generalizations are necessarily based upon the Crinoids as they are represented in our Museums and not upon the Crinoids as they existed in geological time, which is a very different thing." Nevertheless, it is hoped that the present study, fragmentary as it is, may prove of some interest from a phylogenetic point of view.

In 1918, when I gave the results of my first examination of *Eupachycrinus, Zeacrinus,* and *Hydrieonocrinus,* I was not aware of the publication of Professor Wanner's monograph on the crinoid fauna of Timor (*Die Permischen Echinodermen von Timor,* I Tiel, Stuttgart, 1916), in which at p. 153, are given seventeen figures to illustrate the modifications in the anal area of *Cadocrinus* (*Hydrieonocrinus*) *variabilis,* and at p. 170, five figures of *Graphiocrinus timoricus.* Curiously enough, several of these modifications, especially in the former species, may be paralleled among our cups. (See also *Die Permischen Krinoiden von Timor,* Tiel II, The Hague, 1924.) The Timor rocks, containing this fauna, are classed as Permian by the Continental geologists, but this has not hitherto been unreservedly accepted by British and American authors. (Dr. Bather, *Ann. and Mag. Nat. Hist.,* ser. VIII, vol. xiii, Feb., 1914, p. 253; Dr. Springer, *Amer. Jour. Science,* vol. viii, Oct., 1924, p. 325.) However that may be, the Timor crinoids, as a whole, are a highly specialized group, and compared with the crinoids from the British Lower Carboniferous rocks, appear to be in a more advanced stage as regards their anal structures. In this connexion, it is interesting to note that many of the modifications in the anal area of *Eupachycrinus calyx* and *Zeacrinus konincki* foreshadow, or at any rate simulate, the characters which appear to be dominant in so many of the species described by Professor Wanner. (For example, compare *E. calyx,* Figs. 39, 40, 47, 57, and 59 of this paper with *Cadocrinus* (*Hydrieonocrinus*) *variabilis, Cibolocrinus transitorius, C. junkeri, C. propinquis, Ceriocrinus crassus, Erisocrinus malaianus*; or *Zeacrinus konincki,* Figs. 80, 81, 87, with *Parabursacrinus nefotassiensis, P. conus, Graphiocrinus timoricus,* etc.)

EUPACHYCRINUS.

The cups referred to this genus are all placed under one species, namely, *Eupachycrinus calyx* MacCoy. The original name, *Cupressocrinus calyx,* was given by MacCoy to a cup which he says is rare in the Carboniferous Limestone of Derbyshire (*Ann. and Mag. Nat. Hist.*, ser. II, vol. ii, 1849, p. 244; *Brit. Palaeo. Foss.*, 1851, pl. 3D, figs. 1, 1*a*, p. 117). In *Trans. Edin. Geol. Soc.*, vol. x, pt. i, 1911–12, p. 74, Dr. Bather discusses this cup and refers it provisionally to *Eupachycrinus*. He records similar cups, apparently of the same species, from Invertiel, Roscobie, and Clitheroe (specimens in British Museum); from Otterburn and Ryal near Matfen (Newcastle Museum); and from Inverkeithing, Fife (Museum of Practical Geology). He also records in the York Museum a specimen with columnals and some arms attached which may belong to this species. In the same paper Dr. Bather also refers provisionally to *Eupachycrinus*, the cup which de Koninck figured and described as *Hydrieonocrinus scoticus* (*Bull. Acad. Roy. Bruxelles*, ser. II, vol. iv, p. 93, figs. 6 and 7; *Geologist*, vol. i, 1858, p. 179). This cup appears to differ chiefly from *E. calyx* in the actual size of the anal plates, not in their relative position, which is of normal character. In the paper already cited (*Trans. Geol. Soc. Glasgow*, vol. xvi, pt. iii, 1918, p. 376), I placed all the cups therein referred to *Eupachycrinus* under *E. No.* 1 *species*, since I was not then certain that they all belonged to *E. calyx*; but, later, in GEOL. MAG., Vol. LXI, 1924, p. 277, indicated that I thought it probable that all these forms would eventually have to be relegated to *Eupachycrinus calyx*, and that *E. scoticus* was perhaps only a variant of the same species. Considering the modifications which take place in the anal area of the cups under notice, I still think this highly probable.

So far as the genus is concerned, there can be no doubt that the British cups bear a considerable resemblance to *Eupachycrinus*, a name proposed by Meek and Worthen in 1855 (*Proceedings Philad. Acad. Sci.*, p. 159), for the reception of American forms distinguished, among other features, by " the remarkably solid, tumid character of the body pieces ". In 1885 Wachsmuth and Springer (*Revision of the Palaeocrinoidea*, pt. iii, p. 246) state that the cup in all American species of *Eupachycrinus* is depressed, more or less saucer-shaped, the under basals (IBB) are very small and totally covered by the column and rest within a deep concavity together with the lower portion of the basals. This point is further emphasized by Dr. Springer in the Zittel-Eastman *Textbook of Palaeontology*, 1913 edition, where, at p. 224, it is stated that *Eupachycrinus* is similar to *Cromyocrinus* but the calyx is low, rounded, with concave base and infrabasals at bottom of a funnel. (See also Dr. Springer, " Some New American Fossil Crinoids," *Mem. Mus. Comp. Zool. Harvard*, vol. xxv, 1911, No. 3, p. 148.)

The cups dealt with in this communication, which include the 200 already examined, are similar in their general habitus to the British specimens discussed and recorded by Dr. Bather in paper noted above, and that they all belong to the same species I entertain no doubt; but their relation to the American forms is not so clear. If we accept the diagnosis of Wachsmuth and Springer, or the later one of Dr. Springer, it might be argued that the British cups should be placed in a different genus, since in the vast majority of our specimens, considerable portions of the infrabasals are always visible outside the ring of the column facet (see figures and plates accompanying this paper). While in many cases the visible portions of these plates, together with the lower portions of the basals, tend to slope gently downwards towards the cavity of the column socket, in others, the visible portions are more or less flush with the basals which, in their lower portions, are quite flat with no tendency to slope downward. In the latter form, the cups are rather more gently rounded or " flat bottomed " in shape than the former in which all plates of the calyx tend to be more tumid. In only a few instances, such as that shown on Fig. 10 or Fig. 38, the IBB are not clearly discernible since they have very steep sides, almost vertical in fact, from their point of junction with the BB down to the bottom of column socket. It has to be noted that the suture lines between IBB and BB are always easily seen but the lines between the IBB themselves are sometimes very difficult to make out, i.e. from dorsal side of the cup. Probably in such cases, they are fused together. In a few cups, however, which have been cleaned out from the interior, the lines are clearly visible.

The visibility of the infrabasals beyond the column facet and the fact that these plates are not usually sunk in a deep funnel appear to constitute the chief difference between the normal types of our cups and the cups of such American species as *Eupachycrinus formosus* Worthen, or *E. maniformis* Yandell and Shumard, and perhaps also *Zeacrinus cylindricus* Miller and Gurley, which I take to be a *Eupachycrinus* (*Bull. No.* 5, *Illinois State Mus. Nat. Hist.*, 1894, p. 39, pl. iii, figs. 19, 20, 21). The arm structure in these three species agrees with that of our British specimens so far as preserved. Through the kindness of Dr. Springer I have two specimens of *E. formosus* from America. One is a crown, almost complete, the other a cup, and I have compared them carefully with the Scottish material in my collection, consisting of two crowns from Penton Linns, Liddesdale (GEOL. MAG., Vol. LXI, 1924, Pl. XVI, Fig. 4, p. 276), and four more or less complete crowns from No. 3 Bed Invertiel (Pl. XVII, Figs. 4 and 5 of present paper, Text-figure 90). Other than the fact that the IBB are sunk in a concavity and hidden by the column, the cups in this American species have a great resemblance to ours, and so far as the meagre number of specimens allows me to judge, the arm structure is also very similar ; indeed, the description given by Dr. Bather for the specimen in the York Museum might well apply to all, namely, that in each ray the first brachial is axillary and supports two tapering rami of uniserial secundibrachs (*Trans. Edin. Geol. Soc.*, vol. x, pt. i, 1911–12, p. 74). Such facts as these indicate that our British specimens have much in common with typical American forms and generically should in the meantime be retained in *Eupachycrinus*. For the present, therefore, I use this name without qualification and regard *E. calyx* as a typically British species chiefly because the infrabasals are visible beyond the column and so perhaps take up a somewhat greater proportion of the central area of the cup than in American species. Perhaps also, the greater tendency to modification in the anal structures is another characteristic of our species.

CLASSIFICATION.

Statistics for Fife localities are given below :—

INVERTIEL.

Normal Type.	No. 1 Bed . . .	658
	No. 2 Bed . . .	2
	No. 3 Bed . . .	8
Normal Type A.	No. 1 Bed . . .	34
	No. 3 Bed . . .	2
Extreme Type.	No. 1 Bed . . .	95
	No. 3 Bed . . .	6
Extreme Type A.	No. 1 Bed . . .	55
	No. 3 Bed . . .	5
Specimens with one anal (Figs. 57, 58) .		2
Abnormal specimens (Figs. 13, 15) . .		2
Reversed specimen (Fig. 14)		1
Symmetrical do. (Fig. 59)		1
(All six from No. 1 Bed)		
Total		871

ROSCOBIE.

Normal Type	87
Normal Type A	2
Extreme Type	11
Extreme Type A	4
Specimen with 2 anal plates of nearly equal size (Fig. 56)	1
Total	105

SEAFIELD TOWER LIMESTONE.

Normal Type	16
Normal Type A	2
Extreme Type	3
Extreme Type A.	3
Total	24
Grand Total . .	1.000

ABRIDGED TOTALS.

Normal Type . .	771
Normal Type A . .	40
Extreme Type . .	115
Extreme Type A . .	67
Other types . . .	3
Abnormal . . .	3
Symmetrical . . .	1
Total . .	1,000

In my previous examination I divided the 200 cups of *Eupachycrinus* into two main types, the Normal and the Extreme. This grouping, however, does not embrace other important modifications brought to light in the present study, and for our purpose, the following classification is made :—

Normal Type, comprises all those forms in which the radianal is clearly separated from left posterior radial by the anal plate, (x), which is always in close contact with post. basal although it may occasionally be pointed or wedge-shaped and just touches the post. basal ; and with the lower extremity of the radianal in more or less wide contact with right post. basal. This is the primitive type of structure and contains by far the greater number of the specimens. (Figs. 1 to 12, 29.)

As a variant from this, we have a small group, denominated :—

Normal Type A, Figs. 18 to 28, with anal x still in close contact with post. basal but the lower extremity of radianal pointed or wedge-shaped, just detached from the right post. basal and gradually receding from it. The effect here is that the radianal becomes progressively smaller until it culminates in a form like Fig. 27.

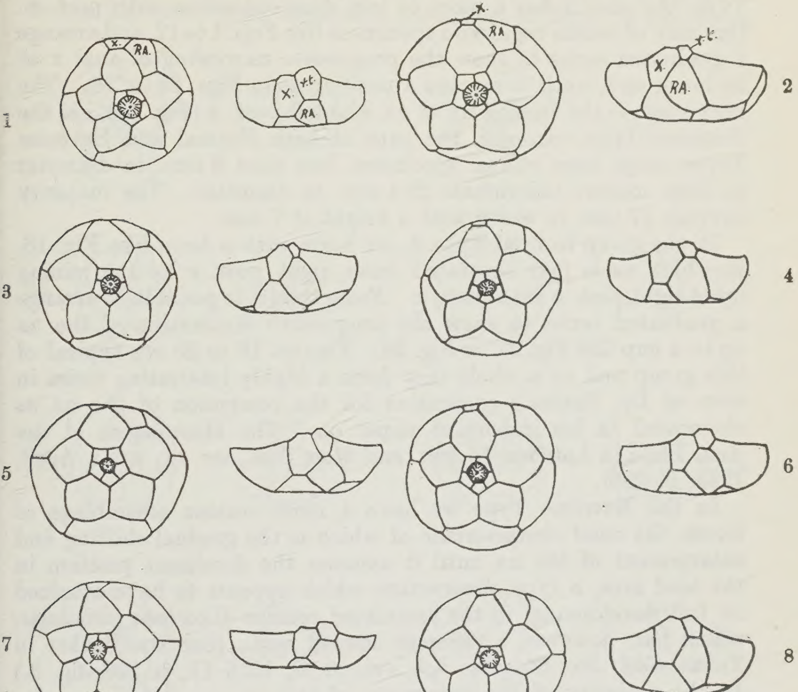
Extreme Type, Figs. 30 to 41, includes all those forms in which the radianal effects a distinct junction with left post. radial, the anal x is pushed upward and point of junction of radianal with left post. radial becomes gradually broader. At the same time, the lower extremity of radianal still has a more or less wide contact with right post. basal, and the plate itself thus gets progressively larger, culminating in a form like Fig. 39 or 40. In most examples of this type, the radianal from pentagonal becomes hexagonal in shape.

Extreme Type A, Figs. 42 to 55, where the lower extremity of radianal narrows and is detached from right post. basal, the effect here being that the radianal becomes gradually smaller until we have a few forms like Figs. 53 and 54.

General Remarks.

As may be noted from Figs. 1 to 59 and Pl. XV, the cups vary considerably in shape ; indeed, one may go as far as to say that scarcely two are precisely alike. The saucer shape predominates, the cups as a whole being somewhat shallow. In others there is a tendency towards a bowl-shaped variety. In some cups, all plates of the patina, as well as the anals, are very tumid, others again are flat and gently rounded. In the tumid variety the central area is more depressed. Features like these often grade into one another, and together with modifications in the anal area, and sometimes the different slope of the radial facets give rise to various outlines when the cups are viewed from the side. For the Normal Type, the general plan of structure is shown on Figs. 1 to 12 with the three anal plates, RA, anal x and *r.t.* always incorporated in the cup, the upper limits of anal x and *r.t.* being outside the line of the radial circlet. While these figures may be taken as fairly representative of the series, there are of course many modifications in the shape of RA, anal x, and *r.t.* When separating the Normal from the

Extreme types for example, such a specimen as that shown on Fig. 30 occasionally caused difficulty. Here, the anal x is triangular and pointed at its lower end, as is also *r.t.* Sometimes it was difficult

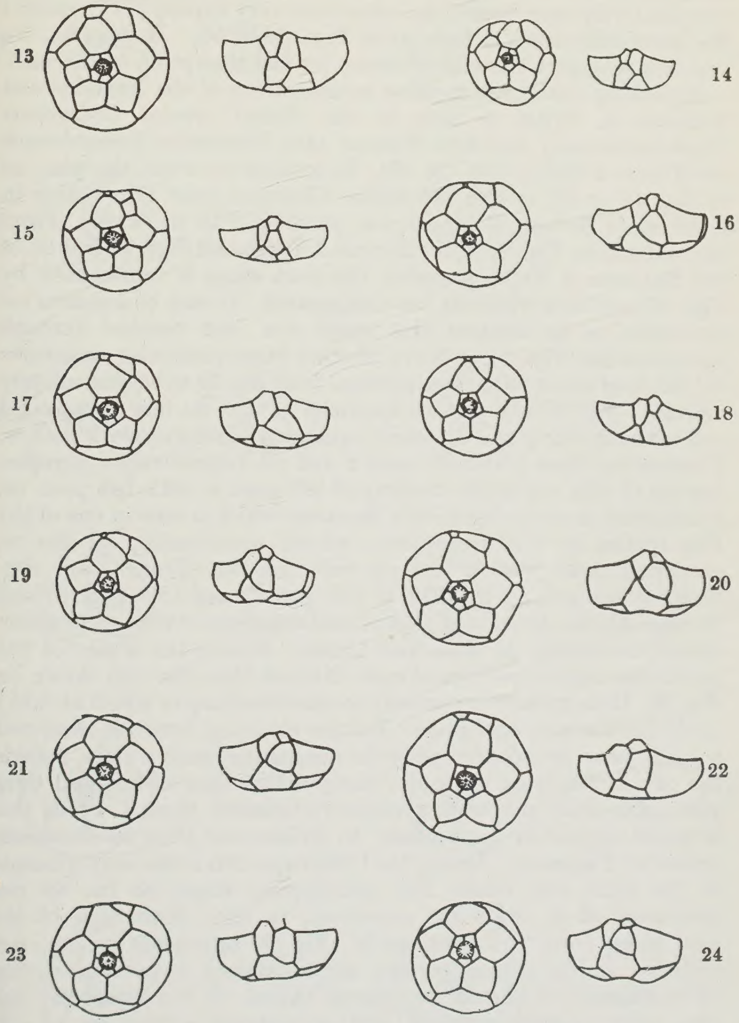


Figs. 1–12.—*Eupachycrinus calyx* MacCoy. A typical series of the normal type. In all these and the following figures of this species a dorsal and side view of each specimen is given. All natural size.

to determine whether the anal X touched post. B, or whether the RA touched left post. R. Such specimens are, however, comparatively few in number. In the great majority of the Normal Type, the anal X has a more or less wide connexion with post. B. One may of course begin with specimens like Figs. 1 to 12, and arrange a graduated series to show the progressive narrowing of anal X at its lower end, until it reaches a position as in Figs. 29 or 30. The next stage is the joining up of RA with left post. R (Fig. 31), i.e. the Extreme Type. In size, the cups of both Normal and Extreme Types range from young specimens less than 5 mm. in diameter to large mature individuals 25·4 mm. in diameter. The majority average 17 mm. in width with a height of 7 mm.

In the group Normal Type A, we begin with a form like Fig. 18, in which RA is just separated from right post. B by the joining up of right post. R with post. B. From this, it is possible to arrange a graduated series to show the progressive diminution of the RA up to a cup like Fig. 27, or Fig. 28. Figures 18 to 28 are typical of this group and as a whole they form a highly interesting series in view of Dr. Bather's contention for the resorption of the RA as elaborated in his important paper on "The Homologies of the Anal Plate in Antedon" (*Ann. and Mag. Nat.*, ser. IX, vol. i, April, 1918, p. 290).

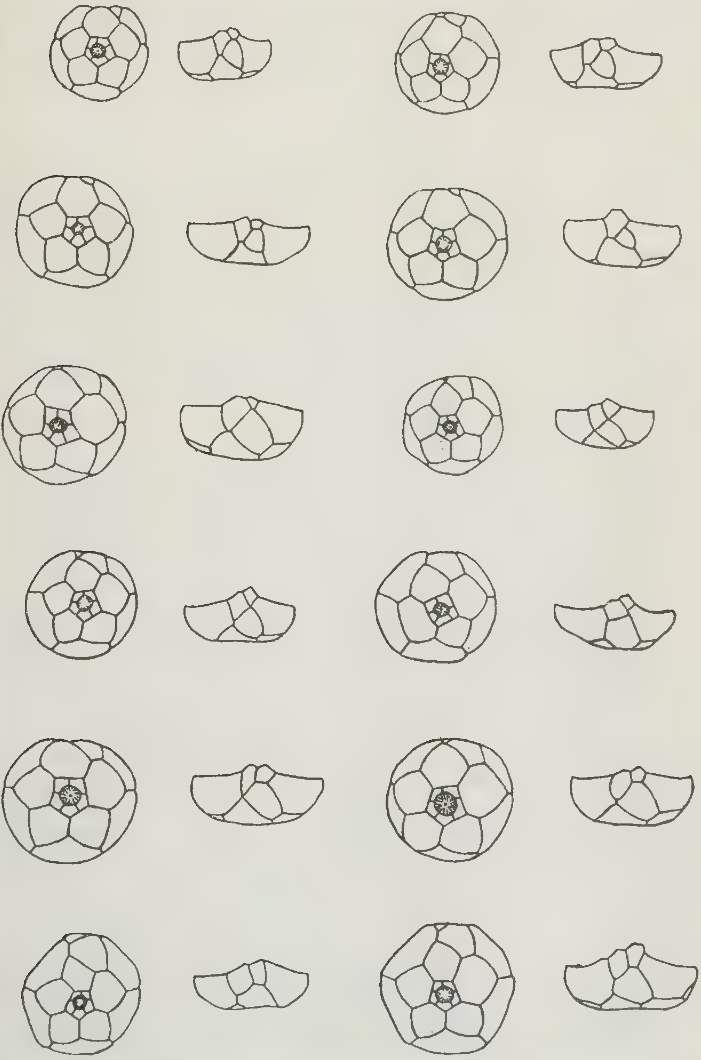
In the Extreme Type we have a most curious assemblage of forms, the chief characteristic of which is the gradual shifting and enlargement of the RA until it assumes the dominant position in the anal area, a type of structure which appears to have attained its full development in the associated species *Ulocrinus globularis*, which has, however, a different shaped cup. (See Dr. Bather in *Trans. Geol. Soc. Glasgow*, vol. xvi, pt. ii, 1916–17, p. 208, fig. 3.) In the majority of the specimens of this species I have so far examined, this character is persistent. In the Extreme Type we may again arrange a graduated series from such forms as Figs. 30 and 31, up to specimens like Figs. 39 and 40. Fig. 39 is somewhat abnormal in that it has six basals, and Fig. 32 four basals. In Fig. 32, it will be observed that *r.t.* is quite detached from RA. In some specimens of the Extreme Type, this feature is even more pronounced than that shown in the figure. In these cases, *r.t.* is a very small plate almost outside the radial circlet, and such modifications appear to be merging into a form like that shown on Fig. 38, in which *r.t.* has passed beyond the cup limits and anal X occupies all the space between left and right post. RR. This figure also illustrates a plan of structure which occurs in several cups of the Extreme Type where anal X is occasionally a smaller plate than that shown in the figure and RA has wide contact with left post. R and right post. B. In fact, they closely resemble the anal structure of *Ulocrinus kaskasensis* as given by Dr. Bather in Lankester's Zoology, Echinoderma, pt. iii, p. 122, fig. xxix. Another feature occasionally seen in cups of the Extreme Type, where RA is a large plate, is that the right shoulder of RA extends up to the line of the radial circlet. In such cases, anal X is a small triangular plate wedged in between left shoulder of RA and top corner of left post. R. In the Extreme Type, from such specimens as Figs. 39 and 40, it



FIGS. 13–24.—***Eupachycrinus calyx*** **MacCoy. Figs. 13 and 15, abnormal specimens; Fig. 14, reversed specimen; Figs. 16 and 17, normal type, showing narrowing of lower extremity of RA, and in Fig. 16, narrowing of lower end of X. Figs. 18–24, specimens of the normal type A. All natural size.**

is possible to arrange a series to show the progressive narrowing of RA at its lower end, indicated by Fig. 41. Through these, we arrive at another interesting group in the Extreme Type A, showing the complete detachment of RA from right post. B and the gradual lessening in size of that plate (Figs. 42 to 55). (Figs. 47 and 48 are comparatively rare forms, the others are very typical of the series.) We eventually reach a form as in Figs. 54 or 55. Here again, we can only interpret these as evidence for the resorption of the RA.

Something analogous to these modifications of the Extreme and Extreme A Types is seen in the Timor species *Cadocrinus* (*Hydrieonocrinus*) *variabilis* Wanner (*Die Permischen Echinodermen von Timor,* 1 Tiel, p. 153, fig. 43). In some of our cups, the plan, as in Fig. 49 or 50, is also not unlike *Ulocrinus buttsi* (Dr. Bather in Lankester's *Zoology*, Echinoderma, pt. iii, p. 122, fig. xxix). From such a form as Fig. 28 in the Normal A Type, and Figs. 49 and 50 of the Extreme A Type, probably the next stage is represented by Figs. 57 and 58 in which RA has disappeared. It may be a matter for discussion as to whether this stage was not reached through specimens like Figs. 51 or 55, or whether these particular structures did not lead direct to the symmetrical form Fig. 59 to be immediately noticed. Fig. 56 is a curious specimen in that RA has disappeared and we have two plates of nearly equal size joining up with post. B. Presumably these plates are anal X and *r.t.* respectively. Another feature of this cup is the meeting of left post. R with left post. IB, reminiscent in some degree of a character which is seen in one of the Fife species of *Hydrieonocrinus*, where, occasionally, all the RR excepting right post. R join up with the IBB (*Trans. Geol. Soc. Glasgow*, vol. xvi, pt. iii, 1918, p. 384, figs. 13 and 18). Figs. 57 and 58 resemble the *Delocrinus* plan of anal structure or that of *Graphiocrinus encrinoides* de Kon. and Lehon. Among the whole of the specimens, none is perhaps of more interest than the cup shown on Fig. 59. Here we have a perfectly symmetrical cup in which at first I could not see any anal plate. Further cleaning, however, disclosed a small plate, or more correctly the remnants of such a plate, outside the cup limits, lying on top of the junction lines of left and right post. RR, and penetrating slightly between them. From this it would appear to come nearer to *Erisocrinus* than to *Stemmatocrinus* or *Encrinus*. Among the 1,000 cups, this is the only example of the kind and marks the culminating stage, so far as our specimens of *E. calyx* are concerned, in the elimination of the anal plates from the calyx walls. Fig. 60 represents a cup, not included in the *Eupachycrinus* series, since I have some doubt as to whether it should be placed there. It is a somewhat flat cup, only slightly crushed, and is interesting because of its exceedingly primitive anal area. Finally, mention should be made of the abnormal specimens shown on Figs. 13, 14, and 15. Fig. 13 has six basals and two anal plates of X character, and Fig. 15 shows two RA. Fig. 14 is perhaps of most interest among these abnormal forms, since it is a left-handed or reversed specimen. This cup is unique so far as *E. calyx* is concerned. In MacCoy's plate (*Brit. Palaeo. Foss.*, pl. 3D, figs. 1, 1*a*) the original specimen is shown in this condition, but this is a fault of the printer, not of Nature.



Figs. 25–36.—*Eupachycrinus calyx* MacCoy. Figs. 25–8, normal type A; Figs. 29 and 30, normal type, showing narrowing of X at its lower end, and Figs. 31–6, the extreme type. All natural size.

In our cup there is no doubt as to its reversed condition; not only so, but I possess another reversed cup belonging to the group which I have referred to, cf. *Cromyocrinus geminatus* Trautschold (GEOL. MAG., Vol. LXI, 1924, p. 277). This cup comes from Ardross, Fife. Dr. Bather has suggested to me that if the interior of the reversed cup of *E. calyx* could be cleaned it might show whether the course of the rectum had also been reversed. So far, however, I have not been able to clean the cup thoroughly, the matrix inside being very hard and difficult to remove without injury to the specimen.

ZEACRINUS.

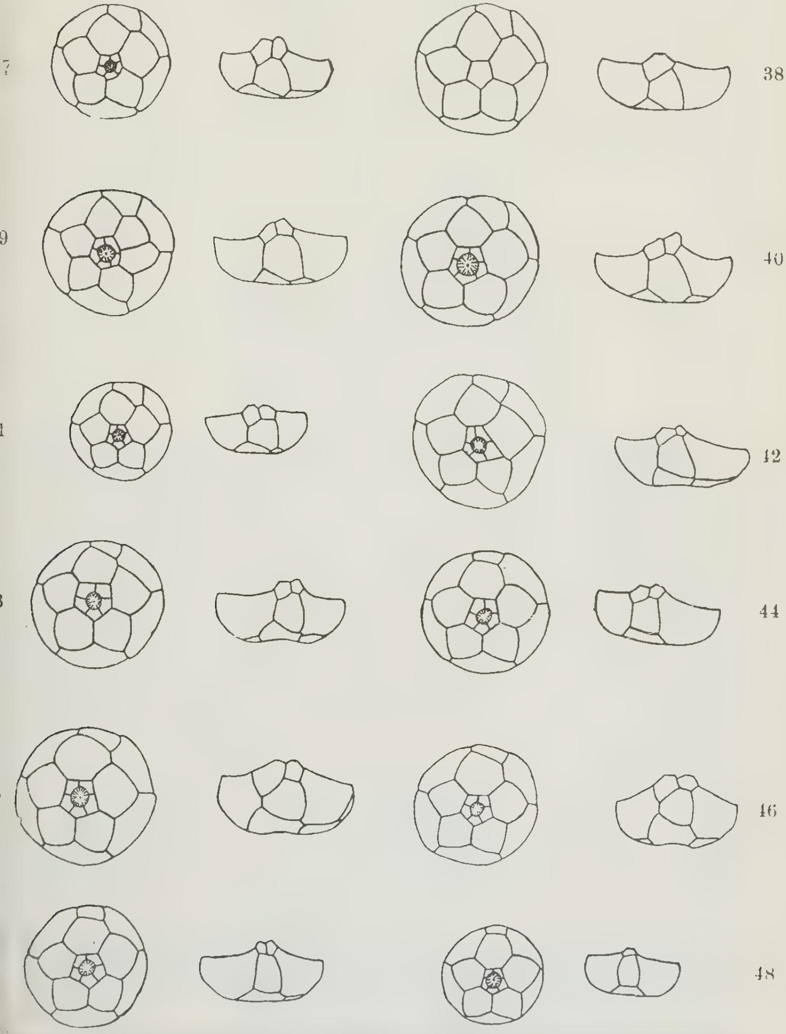
The cups referred to this genus all belong to one species, namely *Zeacrinus konincki* Bather (*Trans. Edin. Geol. Soc.*, vol. x, pt. i, p. 73). In this paper Dr. Bather refers MacCoy's species *Cupressocrinus impressus* (*Ann. and Mag. Nat. Hist.*, April, 1849, p. 244; *Brit. Palaeo. Foss.*, pl. 3D, figs. 2, 2*a*) to *Eupachycrinus* ? *impressus*. In *Trans. Edin. Geol. Soc.*, vol. xi, pt. iii, 1925, p. 287, I alluded to this cup and gave reasons for thinking it should be placed in *Zeacrinus*. Some cups which I have examined in connexion with this paper further support this view, but, being few in number, they do not concern us meantime.

The cups of *Zeacrinus konincki* here dealt with number 342. We have in this species an interesting series of forms showing modifications in the anal area not unlike those already noted for *Eupachycrinus calyx*. So far as the actual number of cups is concerned, however, these modifications are in a somewhat different ratio. Because of this the classification here adopted is somewhat different. In the holotype of *Z. konincki*, as figured by Dr. Bather, the general plan of anal structure is similar to that shown by such specimens as Figs. 67 to 72 of present paper, where the RA is a large plate and makes contact with left post. R, and with anal X more or less widely separated from post. B. This is the type of structure shown in the greater number of our specimens. One may of course notice many modifications in the size and shape of RA, which may have a wide contact with left post. R, and the consequent pushing upward of the anal X; or it may be quite narrow here with anal X lower down in the cup. (The anal X itself also varies, as may be noted in the figures.) The same applies to the lower end of RA. In many specimens its junction with right post. B is wide, in others narrow, and this leads up to another series in which RA becomes detached from right post. B and right post. R joins up with post. B. Through a series of modifications of this kind we eventually reach a form like Fig. 88, in which RA is almost resorbed and the other anal plates have disappeared from the cup. Apart from these, we have a small group with a more primitive anal area where anal X is linked up with post. B, corresponding in this respect with the Normal Type of *Eupachycrinus calyx*. This group being the smaller, the classification is as follows:—

Primitive Type, Figs. 61 to 65, includes all forms in which anal X, at its lower end, has more or less wide contact with post. B.

Primitive Type A, as above, but with lower end of RA detached from right post. B. (Fig. 66.)

Advanced Type, a series of forms like Figs. 67 to 74, which differ but little from the holotype, excepting that RA, anal X, and *r.t.* may vary considerably as regards their size and shape.



Figs. 37–48.—*Eupachycrinus calyx* MacCoy. Figs. 37–41, the extreme type; Figs. 42–8, the extreme type A.

Advanced Type A, a modified form of the advanced type, in which RA is detached from right post. B, becomes smaller and eventually, along with anal X and *r.t.*, almost or entirely disappears from the cup (Figs. 75 to 88).

Statistics for Fife localities are given below :—

Primitive Type.	No. 1 Bed, Invertiel	40
	Roscobie	5
	Seafield Tower, Limestone	4
Primitive Type A.	No. 1 Bed, Invertiel	5
Advanced Type.	No. 1 Bed, Invertiel	186
	Roscobie	26
	Seafield Tower Limestone	19
Advanced Type A.	No. 1 Bed, Invertiel	55
	Roscobie	1
Abnormal		1
	Total	342

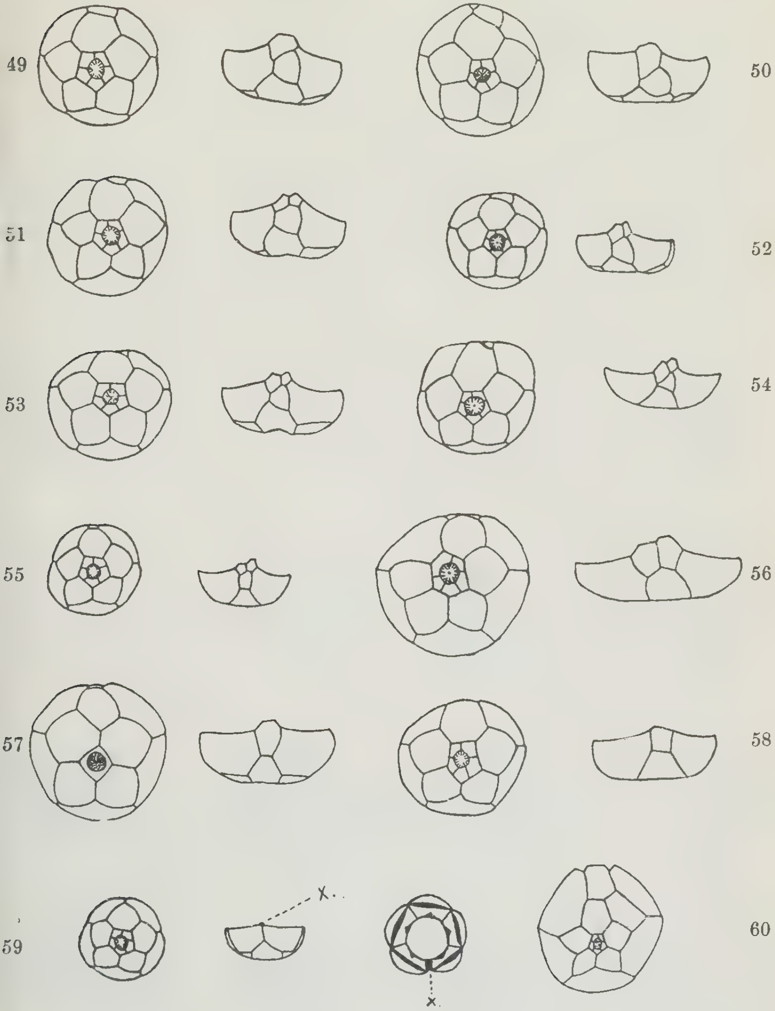
ABRIDGED TOTALS.

Primitive Type	49
Primitive Type A	5
Advanced Type	231
Advanced Type A	56
Abnormal	1
Total	342

GENERAL REMARKS.

As a whole, the cups of *Zeacrinus konincki* are fairly uniform in shape, and, as may be seen from Plate XVI, are more or less flat with the central area impressed. From near their junction with the RR, the BB slope gently downwards into a shallow pit at the bottom of which are the IBB. As compared with *Eupachycrinus calyx*, the radials occupy a relatively greater proportion of the cup, the basals being comparatively small plates and the infrabasals invisible when the proximal columnals are in position. One observes, however, that the BB themselves vary in size even in cups of similar dimensions. When the BB are smaller than usual, they tend to slope more steeply towards the column socket, and thus give the cup a rather more rounded appearance, since greater proportions of the RR are brought down with them. The modifications in the anal area also help to slightly alter the contour of the cups. In specimens of the nature of Figs. 86 and 87 the more rounded outlines are noticeable. Here, of course, both BB and IBB form a comparatively small part of the cup. For the sake of uniformity, the figures are drawn to show the infrabasals, but in many cups it is almost impossible to see them, and I think it probable in some cases that these plates, if not entirely resorbed, are almost so. In size the cups vary from young specimens less than 5 mm. in diameter to large individuals 23 mm. in diameter, i.e. measured across the widest angles of the cup. An average size measures 18·8 mm. wide with a height of 5 mm.

In the Primitive Type, represented by Figs. 61 to 65, we have a relatively small group with the three plates, RA, X, and *r.t.* well within the cup limits, anal X meeting post. B in more or less wide contact. As in the Normal Type of *Eupachycrinus calyx*, the lower end of X

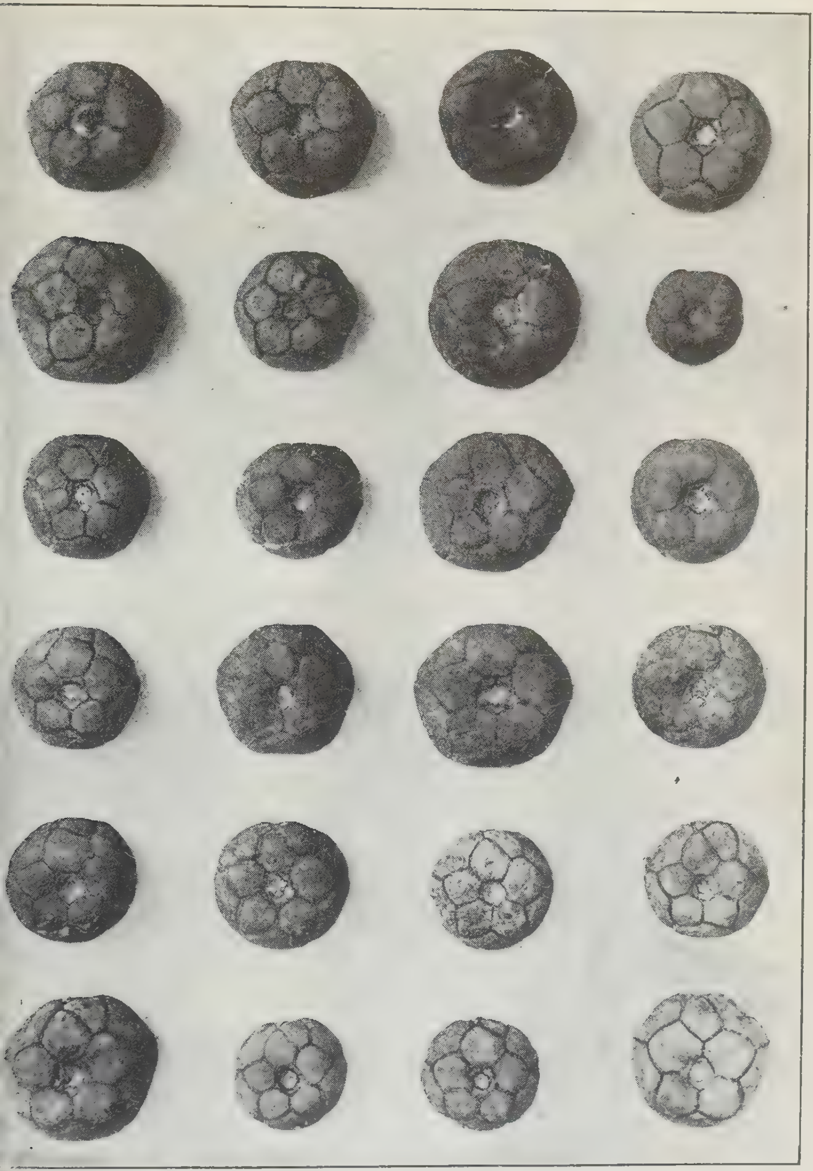


Figs. 49–59.—*Eupachycrinus calyx* MacCoy. Figs. 49–55, the extreme type A; Fig. 56, specimen with two anals meeting post. B. Figs. 57 and 58, specimens with one anal, RA, resorbed; Fig. 59, symmetrical cup, dorsal, side, and ventral views. In the side view the remnant of anal plate is shown rather more prominently than in the specimen. Fig. 60, a cup doubtfully referred to *Eupachycrinus calyx*. All natural size.

may be pointed, just touching the post. B, and occasionally difficulty is experienced in determining whether RA joins left post. R or not. In this type we may arrange a graduated series to show the narrowing of RA at its lower end, which leads to the Primitive Type A, in which we have only five specimens, showing RA detached from right post. B, while at the same time anal X still holds contact with post. B. Fig. 66 illustrates one of these specimens, and it may be observed in this case anal X just touches post. B.

In the Advanced Type (Figs. 67 to 74) the series corresponds with the Extreme Type of *Eupachycrinus calyx*, and the same arrangement can be made here, namely, a graduated series to show the progressive widening of line of contact of RA with left post. R until it reaches a stage as in Fig. 74. From this we may then arrange a group to show the narrowing of lower end of RA until it becomes detached from right post. B, bringing us to the Advanced Type A, which is perhaps the most interesting from the evolutionary point of view. Here, however, the ratio is considerably less than that of the corresponding forms of *Eupachycrinus calyx*. At the same time, the series is no less instructive since we see here the progressive diminishing of the RA until it disappears from the cup (Fig. 87). Fig. 88 shows the remnant of RA almost on the point of being resorbed, while the anal X has passed outside the cup limits. In this specimen there is a small notch between left and right post. RR and examination of the interior of the cup shows that it was occupied by a very small plate, apparently of X character. Fig. 87 closely resembles one of the modifications in *Graphiocrinus timoricus*, as figured by Professor Wanner (1916). Fig. 81 represents a cup of which there are several examples, the RA being a long plate with anal X and *r.t.* outside the line of the radial circlet. Figs. 75, 76, 77, 78 resemble the plan of anal structure given for the cup of *Zeacrinus peculiaris* Miller and Gurley (*Bull. No.* 8, *Illinois State Mus. Nat. Hist.*, 1906, pl. ii, fig. 19). (Compare this with *Paraburaacrinus conus* Wanner.)

In the figures only a few typical examples of the different types are given, since time and space do not permit of further details at present. From the ratio of the total numbers, however, it may be observed that as regards its anal structure *Zeacrinus konincki* is in a more advanced stage than *Eupachycrinus calyx*. The arm structure is also much more complicated, as is beautifully shown in a series of well-preserved crowns which I possess from Invertiel and Roscobie. Later, it is proposed to work out these crowns in conjunction with the cups already noted. It is an interesting speculation whether the modifications in the anal area, not only of *Z. konincki* but also of *E. calyx*, may not have been correlated with variations in the arm structure. This may be possible to ascertain, to some extent, in the case of *Z. konincki*, but seems rather too much to hope for in the case of *E. calyx*, since crowns of this species are excessively rare. Meanwhile, as an indication of the arm structure in *Z. konincki*, I give a drawing of specimen No. 428 from Roscobie (Fig. 89, Pl. XVII, Figs. 1



rright, Photo.

Eupachycrinus calyx MacCoy.

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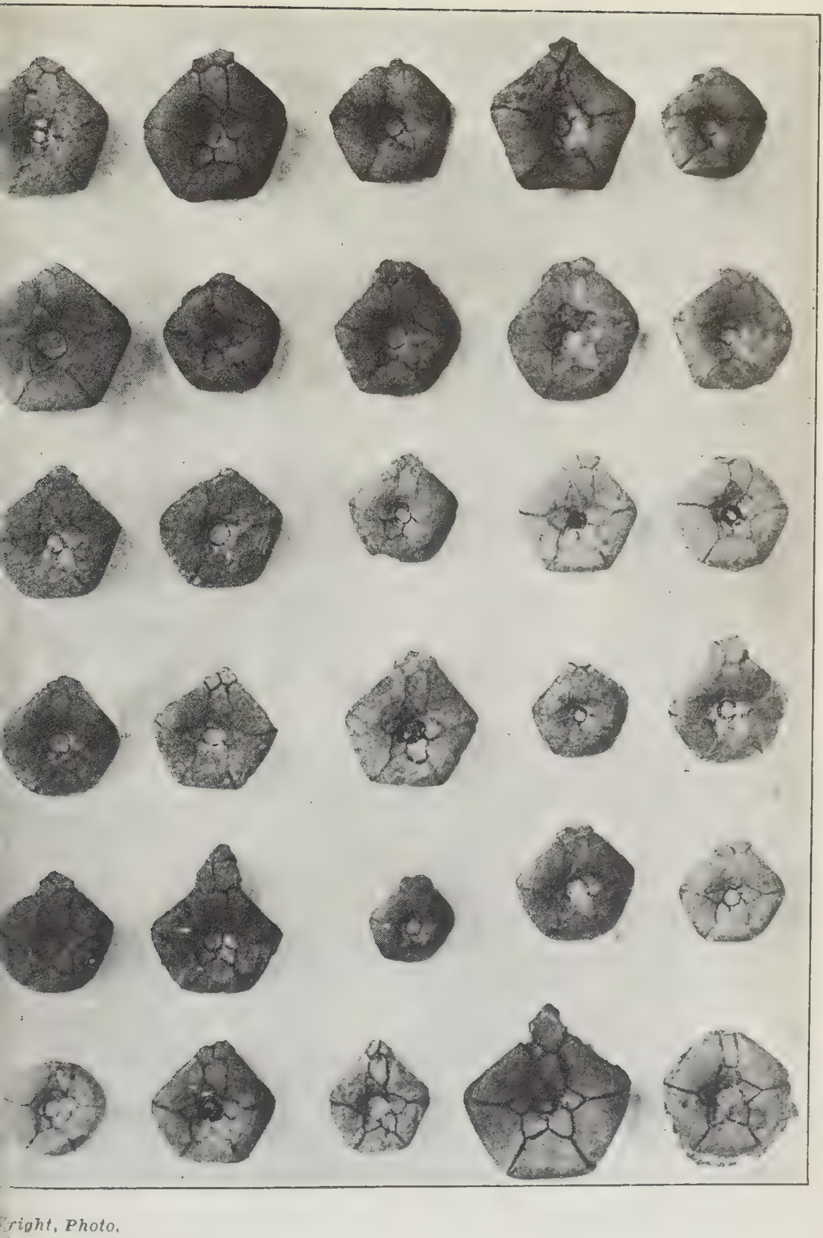

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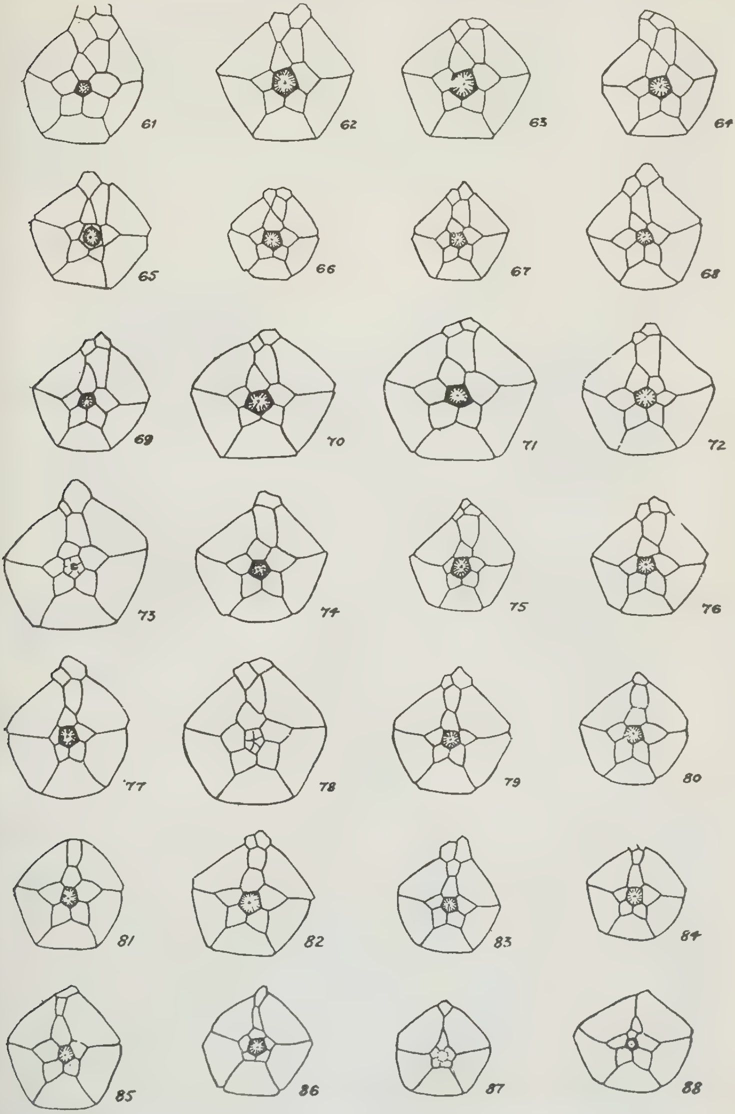
ZEACRINUS KONINCKI BATHER.

and 2), which is the finest preserved specimen of this species I have ever seen. For comparison a diagram of the arm structure of *E.*



Figs. 61–88.—*Zeacrinus konincki* Bather. Figs. 61–5, primitive type; Fig. 66, primitive type A; Figs. 67–74, advanced type; Figs. 75–88, advanced type A. All natural size.

calyx is also given (Fig. 90, Pl. XVII, Figs. 4 and 5). These will serve to show the difference in the arm structure in the two species.

As to the occurrence of the cups of *E. calyx* and *Z. konincki* in Fife, we have one of those mysteries in the distribution of crinoids which are so difficult to explain. Both species occur in fair numbers in the Seafield Tower Limestone ("Hurlet" of Survey Memoir, 1900), of the West Fife area. Although carefully searched, none of the limestones, above or below this horizon on the Kirkcaldy-Kinghorn coast section, have so far yielded a single specimen of either species. In the East Fife area at St. Monans, 20 miles or so distant, the Lower Carboniferous is again splendidly exposed. One of the limestones here must be the equivalent of the Seafield bed, and yet not a solitary specimen belonging to either species has ever been found in any of these deposits; although cups and crowns belonging to other species which are closely associated with *Z. konincki* and *E. calyx* in the West Fife area have been found in fair numbers at three separate horizons. Such facts are interesting but puzzling. The cups here dealt with therefore all come from one horizon, i.e. the Seafield Tower Limestone or its equivalent at Invertiel and Roscobie. Several cups of these species which I have recorded from other localities where the same limestone is exposed are not included in above totals, since they are comparatively few in number and not

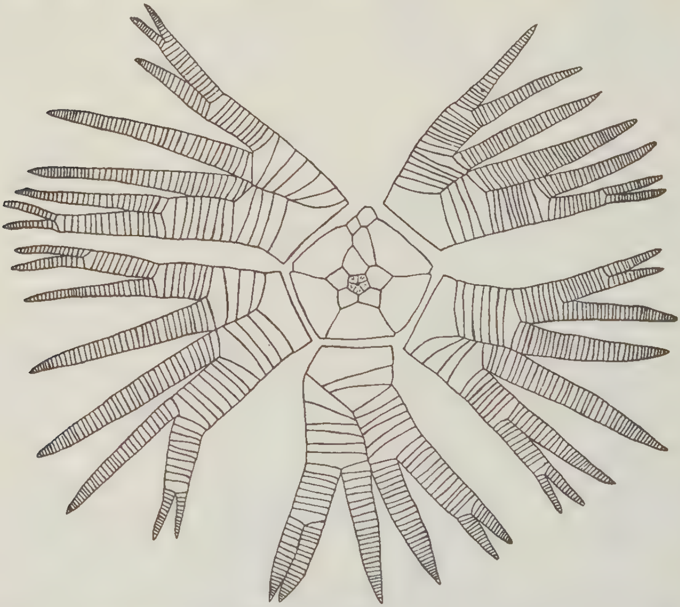


Fig. 89.—*Zeacrinus konincki* Bather. Specimen No. 428, author's collection, from Roscobie. Complete crown to show arm structure. Natural size.

so well preserved. Later, it may be possible to deal with them and a few others from Nos. 2 and 3 Beds, Invertiel, not yet looked out.

SUMMARY OF THE TWO SPECIES.

Eupachycrinus calyx.		*Zeacrinus konincki.*	
Normal (Primitive) Type .	771	Primitive Type . . .	49
Normal (Primitive) Type A.	40	Primitive Type A . .	5
Extreme (Advanced) Type .	115	Advanced Type . .	231
Extreme (Advanced) Type A	67	Advanced Type A . .	56
Other types . . .	3	Abnormal . . .	1
Abnormal . . .	3		
Symmetrical . . .	1		
Total . . .	1,000	Total . . .	342

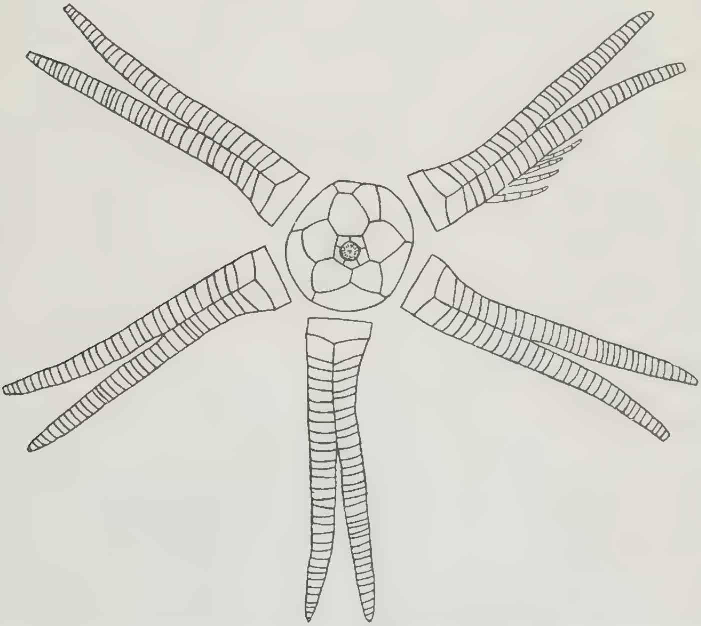


FIG. 90.—*Eupachycrinus calyx* MacCoy. Diagram of the arm structure of this species. Based on several specimens. Natural size.

For the purpose of this paper 115 cups of *Eupachycrinus calyx* and 35 of *Zeacrinus konincki* were photographed and drawings made direct from the prints. An attempt has been made to show the more interesting of these. Of course, the classification here adopted is a purely arbitrary one, but that it holds a relationship to the evolutionary trends there seems little doubt. Further comment at this stage would perhaps be premature, but from the study so far as it has gone, especially that of *Eupachycrinus calyx*, there would appear to have been three directions in which the evolutionary

Processes acted: (1) from the Normal or Primitive Type direct by the gradual resorption of RA; (2) from the Normal or Primitive Type by the joining up of RA with left post. R and the gradual increase in size of RA until it attained a dominant position in the anal area: from this stage (3) the gradual diminution of RA. In respect of reduction of RA, (3) is parallel to (1). Through these we eventually reach a plan of anal structure simulating *Delocrinus* and *Graphiocrinus*, and finally *Erisocrinus* in which the anal plates are eliminated from the calyx. A not dissimilar state of things is shown in *Zeacrinus konincki*, which, however, had apparently reached another stage on the path of evolutionary progress.

EXPLANATION OF PLATES XV–XVII.

FIG.

PLATE XV.

Eupachycrinus calyx MacCoy. A typical series of cups. Nat. size.

PLATE XVI.

Zeacrinus konincki Bather. A typical series of cups. Nat. size.

PLATE XVII.

1.—*Zeacrinus konincki* Bather, Specimen No. 428, author's collection, anal inter-radius to left.

2.—The same specimen from anterior.

3.—Specimen No. 1662, author's collection, to show arm branching. This and the preceding specimen are from Roscobie.

4 and 5.—*Eupachycrinus calyx* MacCoy, specimens Nos. 1536, 1542, author's collection from No. 3 Bed, Invertiel; somewhat crushed crowns.

All nat. size except Fig. 3, which is $\frac{3}{2}$.

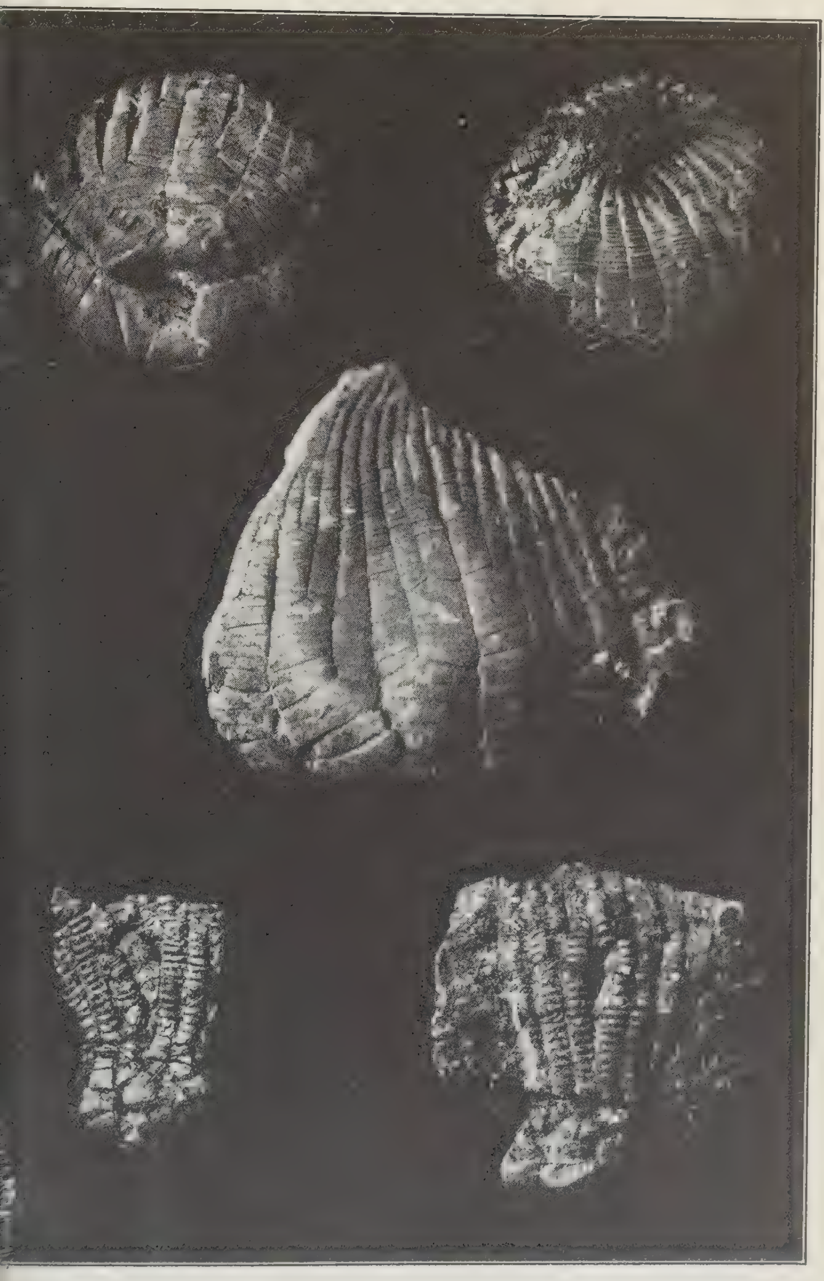
The Formation of Rock Basins.

By J. D. KENDALL.

IN 1880 I communicated to the Manchester Geological Society a short paper, on the above subject, which endeavoured to show that Ramsay's suggestion of glacier erosion cannot explain the origin of Rock Basins. I have often since considered the facts and arguments advanced by different writers on the subject, but am more convinced than when I previously wrote that our Lake Basins were not formed by glacier erosion. The arguments herein are additional to those employed in my communication to the Manchester Geological Society.

I now propose to confine my remarks to the English Lake District, and in detail, particularly to the rock basins in the Southern part of it. Considerable glacial data were collected by the late J. C. Ward with reference to the greater part of the Lake District, when he was engaged in preparing a general map thereof on behalf of the Geological Survey. I have worked in the district on glacial and other geological subjects many years.

Prior to a study of Lake- and Tarn-making it is necessary to reach an approximate idea of the erosive work done by glaciers in the district. As we ramble among its mountains frequent



2

3

5

right, Photo.

ZEACRINUS AND EUPACHYCRINUS.

[To face p. 164.

evidences are seen of glacier erosion—the up-valley sides of rocks are rounded and striated, whilst the down-valley sides are, as a rule, jagged and irregular, as shown in Fig. 1, which appeared in my paper on "The Glacial Deposits of West Cumberland".[1] But there is not any evidence that these rubbings were anything like so widespread or deep as was at one time supposed. A part of the rounded and angular fragments of rock included in the Boulder Clays and in the sands and gravels, spread out unevenly over the lower ground surrounding the Lake District, was doubtless derived from the mountain sides that stood above the level of the glaciers, and was deposited on the ice as lateral and medial moraines. These were carried out seaward, first by the glaciers and finally by bergs, and deposited in the Boulder Clays formed on the sea bottom in front of the glaciers. Many fragments doubtless fell through the glaciers on their way out and were rounded, scratched and broken up on the glacier bed. These effects, of course, were only in part due to glacier erosion. A proportion of the rounded and angular fragments in the Boulder Clay and a large part of the clayey matrix of that deposit were certainly due to ice action on the rocky base over which the glaciers moved. Irregularities of the underlying surfaces would be reduced and rounded and scratched faces produced, as in Fig. 1, on the up-valley side of prominences.

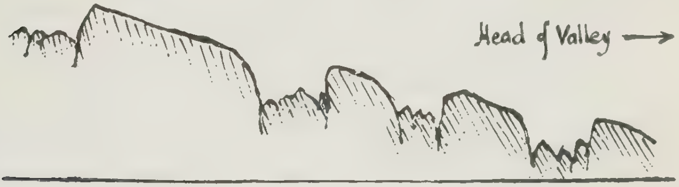


Fig. 1.

It is impossible to get exact figures, but an approximate idea of the weight of material existing as boulders, gravel, sand and clay and which must have been carried or dragged seaward and deposited by the glaciers, and the water flowing under them, on to the low-lying ground surrounding the mountains, I think are of importance in this inquiry. The Lake District may be said to cover an area of about 900 square miles, but an increase due to mountain surfaces would make the area exposed to glacier action at least 1,400 square miles. The area of the ground surrounding a large part of this mountainous tract may also be taken at about 900 square miles. The glacial deposits scattered over that area may have an average thickness of about

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[1] *Trans. of the Cumberland Association for the Advancement of Literature and Science,* part vii, 1881–2.

25 feet. That means that denudation removed from the interior or mountainous district an average thickness of about 16 feet. But probably half of that quantity would be due to superglacial moraines and the sand and mud carried through the ice from the hill-sides above the glaciers. There would consequently be a removal by sub-glacier action of probably not more than 7 feet. That does not seem much when spread over the long glacier period, but all my observations point to the conclusion that the actual quantity broken or rubbed from the glacier-bed by ice was probably less than that.

Lakes and tarns in rock basins invariably occur on level parts of the valleys in which they are located, that is, the valleys longitudinally would be level or nearly so for the full length of the future tarn or lake. Neither of these water-mass occurrences could exist if the valleys at the outlet-ends of the lakes and tarns had not been as high as the inlet-ends. This is an essential condition, and it will be referred to later when we reach the core of the subject. There cannot, in my view, be any doubt that those parts of valleys, where lakes and tarns exist, were more or less level in those lengths when the basins began to be formed.

In order that the subject may be viewed at close quarters I propose to deal with the lakes and tarns situated in the southern part of the Lake District, including Wastwater, Coniston Lake, Esthwaite Lake, Grasmere, Windermere, and Rydal Lake. J. C. Ward dealt with the same area in his paper on the Glaciation of the Southern part of the Lake District.[1] I have utilized some of his observed ice scratches and have added others seen by myself. They do not cover the whole area examined, but I think they are sufficient for the inquiry.

Figs. 2, 3, and 4 relate to Wastwater and various parts of its rainwater basin, and Figs. 5, 6, and 7 refer to Windermere and the other four lakes—Grasmere, Rydal, Coniston, and Esthwaite—in the same drainage area. These show that the valleys at the head of and adjoining the sides of the lake-sites were occupied by ice. Fig. 6 shows a large body of ice extending from Wetherlam to Wansfell, as suggested by the ice-marks. This indicates that much of the ice from the Pike of Blisco and Wetherlam and possibly the upper part of the ice from Langdale Pikes would go southward over the low ground towards Esthwaite Lake and the foot of Windermere and would not go towards the head of Windermere as might, at first, be supposed. The ice-scratches are not always a guide to average ice flow when they occur on and near the bottoms of the valleys. They may indicate the course of the retreating glaciers and, at any time, of only the bottom ice. From the evidence of the ice-marks it appears the glacier from Codale Tarn via Grasmere to the south of Belle Isle, near the middle of Windermere (Fig. 5), probably had a surface gradient of about 2 degrees. The glacier (Fig. 7) from Coniston Old Man, Wetherlam, Pike of Blisco, and Langdale Pikes via Esthwaite to Windermere near Rawlinson's Nab, and then down the lake, probably had a similar surface grade. There was no doubt a like surface inclination of the glacier from Great Gable to the head of Wastwater, and then down the lake (Fig. 2). This rate of incline is adopted because it would be about that produced by the onward movement of the glaciers and the gradual lowering of their surfaces by ablation and the subsidence caused by subglacial melting. Moreover, it agrees with the lowering altitude of the highest ice-scratches as they occur down the valley. Two degrees is about one in twenty-six. The inclinations adopted by Ward are, from Codale Tarn over Windermere about 1 in 80, and from Great Gable over Wastwater 1 in 37 or hereabouts. He gives the thickness of the ice at the foot of Windermere as 900 feet and about the same at the foot of Wastwater.

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[1] *Quart. Journ. Geol. Soc.*, vol. xxxi, p. 152.

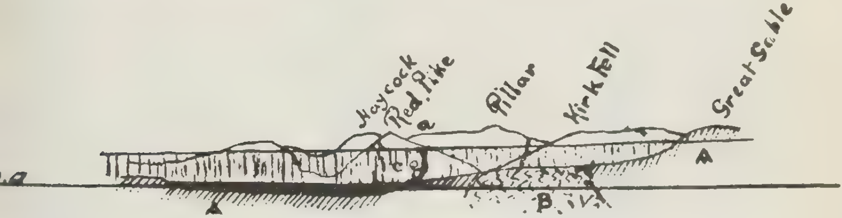

FIG. 2.—A, Borrowdale Volcanics; B, Eskdale Granite; *a*, falling water. Scale, ½ in. = 1 mile.

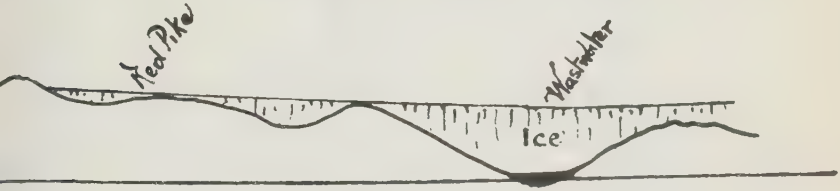

FIG. 3.

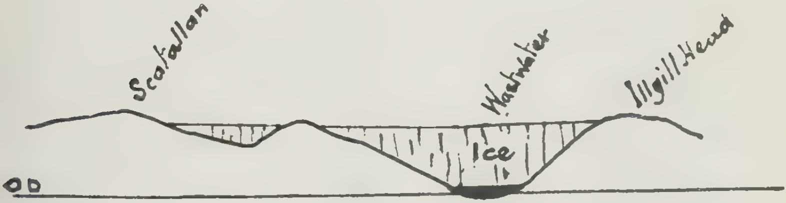

FIG. 4.—Scale, 1 inch = 1 mile.

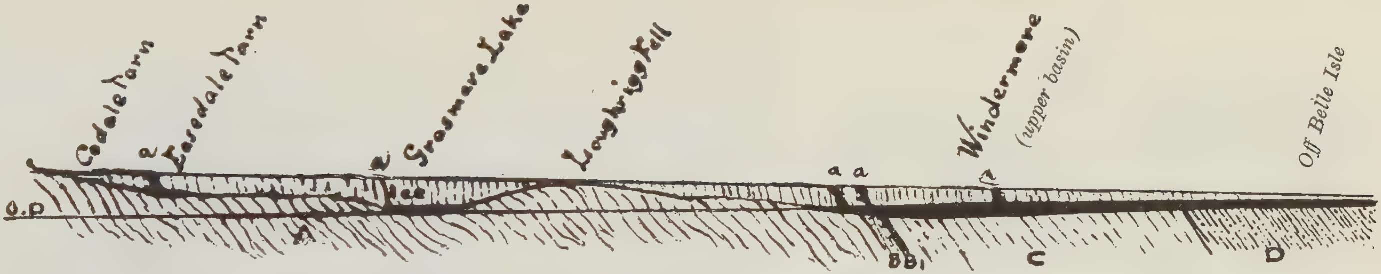

FIG. 5.—A, Borrowdale Volcanics; B, Coniston Limestone; B_1, Stockdale Shale; C, Coniston Grits and Flags; D, Bannisdale Series (sandstone and shales); *a*, falling water. Scale, ⅔ in. = 1 mile.

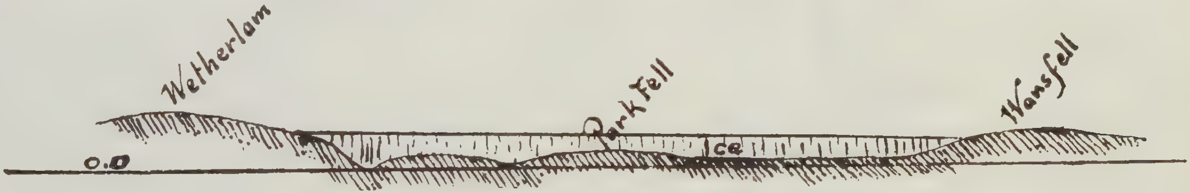

FIG. 6.—Scale, ⅔ in. = 1 mile.

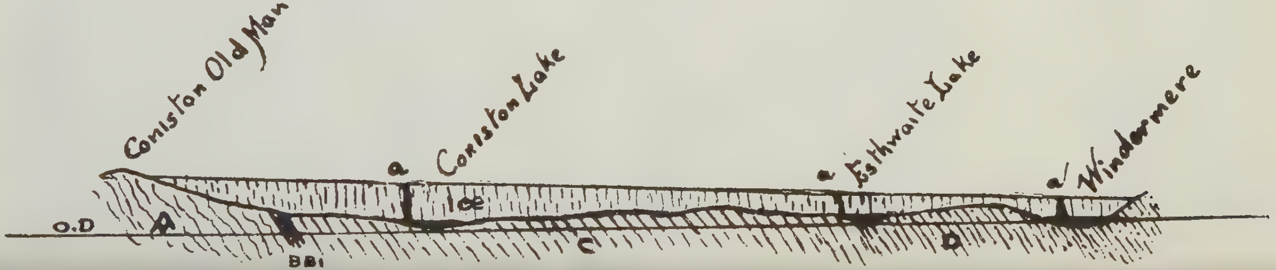

FIG. 7.—A, Borrowdale Volcanics; B, Coniston Limestone; B, Stockdale Shale; C, Coniston Grits and Flags; D, Bannisdale Series

The masses of ice which formed on the upper parts of the Wastwater basin at Scawfell, Scawfell Pikes, Great Gable, Kirk Fell, Pillar, and Haycock would move along the valleys towards the lake and fill the former until the surface gradient, above mentioned, was reached. The unmelted ice would move partly over the lake-site and partly over each side of that area. The melted ice would nearly all flow over the lake-site. From the high ground on the east, west, and north of the basin in which the other five lakes occur, the ice, formed on the basin area, would move partly down the Vale of Troutbeck, also from Raven Crag and Calfcover, from Steel Fell and the high ground above Codale Tarn, from Langdale Pike and Pike of Blisco through Great Langdale towards Windermere, some of the ice from Codale Tarn would pass over Grasmere Lake and Rydal Water. The combined glacier would then move down Windermere to the south of Belle Isle. Another ice-stream would move from the Pike of Blisco, Wetherlam, and Coniston Old Man partly over the site of Coniston Lake and partly over that of Esthwaite Lake to the lower Windermere area, terminating south of the present foot of that lake. The melted ice would pass over the different lake-sites in greater or less proportion, according to the area of the watershed above them. The ice moving over the lake-sites would be only a small part of that formed in the basin.

Ward was troubled by the existence of islands near the middle of Windermere. Like Ullswater, Windermere has two basins. In neither case could two basins have been formed if only one glacier had moved from end to end of each lake. Windermere from Hen Holme to near Storr's Temple is shallow, and there are several islands.

Of the ice formed within the hydrographical area in which the lakes occur, a large proportion of it would be converted into, and pass down the drainage area, as water of ablation, etc. Probably less than one-tenth of the ice originating in the basin would pass over the lake-site or lake-sites on its way out of those areas, but the whole of the water produced on the areas above each lake-site would flow over the lake-site or lake-sites at lower levels in the valleys, and on account of the low winter temperature, it would nearly all flow off during the summer months. The area of the glaciers moving towards the head of the upper basin of Windermere would be about 45 square miles. That moving from Coniston Old Man and Langdale Pikes, etc., partly over Coniston Lake and afterwards, via Esthwaite Lake to Windermere lower basin would have an area of about 32 square miles, and that of the Wastwater glacier about 19 square miles. These enormous bodies of ice would be changed on the surface during summer into large volumes of ablation water. To a less extent water would also be formed at the bottom of the glaciers. The average thickness of ice might be about 800 feet. It would probably be reduced at the rate of 12 feet a year, perhaps more or less, but I put it at that figure. We have not any precise information as to the variations of temperature and snow- or rainfall, but I assume the winter weather lasted about six months and that the precipitation was similar to what it is to-day. The melting of the ice would mostly occur during the summer. The quantity of ablation water may be taken as equal to a rainfall during 180 days (the summer period) of about 144 inches, or about ·8 inch per day, which is probably about five times the quantity of rain falling annually on these areas in our days. As a denuding agent the excess then over our present rainfall would be greater still by perhaps ·2 inch per day as there would be little loss by absorption and evaporation. That enormous quantity of water would flow over the sites of Wastwater and Windermere and, to a less extent, over the sites of Grasmere Lake and Rydal Water.

A similar argument may be applied to the glacier formed on the upper part of Langdale Pikes, Pike of Blisco, Wetherlam, and Coniston Old Man, part of the water from it flowing towards the head of Windermere, other portions flowing over the site of Coniston Lake, Esthwaite Lake-site, and finally over the lower basin of Windermere.

The large quantity of ablation water flowing along the surface of the glaciers would be intercepted by crevasses and moulins, down which it would fall with great force, on to the rocks below, in consequence of the great thickness of the ice-masses. Assumed waterfalls are shown on the sections at a few points. The denuding effect of that water combined with that of the rock fragments falling from the moraines must have been very great. When they fell on ground having a considerable inclination there would not be any tendency to form a lake or tarn as the water and accompanying rock-fragments would have a free outlet below, but on practically level ground, like that where the lakes now exist, the formation of a basin would be a certainty. It has been urged that the falling streams could not keep in motion the large bodies of water forming the lakes. But that is assuming something that never existed. The water flowing under the glacier would not have a free top, as it has in Fig. 8, which is a section of Selcoth Burn. At the beginning there would only be the stream water and its rock inclusions flowing from the melting glacier, between a greater or less part of the bottom-ice and the underlying rock. As the enormous quantity of falling water and rock-fragments, during many hundreds, perhaps many thousands of years continued their disrupting and abrading actions on the underlying rocks, the latter would be dug into and rubbed lower and lower, the action being greatest where, owing to the configuration of the ground, the ice was extensively ruptured, as for example at the junction of valleys, where water and rock-fragments would be let down in destructive quantities. At the head of Grasmere Lake and Rydal Water the ruptured glaciers would form a downward path for the falling water and rocks. The same would occur when the converging glaciers came together near the head of Windermere, and also at Calgarth, where the main glacier was joined by that from Troutbeck. The rupturing of the ice at these points would arise from the different velocities and courses of the combining glaciers.

The lower basin of Windermere would be partly worn by the water and rock-fragments flowing from the upper basin, but it would mainly be formed by the water and rock-fragments falling through the lower end of the glacier that came from Coniston Old Man, Wetherlam, and Pike of Blisco—Esthwaite Lake and Coniston Lake having been formed by the water falling through the glacier on the more or less level ground existing at those places. The ice formed in the neighbourhood of the above-mentioned three sources, in moving towards Windermere, would be ruptured at the head of Coniston Lake and also at the head of Esthwaite Lake and at Windermere where part of the ice moved eastward from Esthwaite and a large part made an important turn southward down the lower basin of Windermere. Numerous crevasses would also occur elsewhere, down which water and rock-fragments would fall and add to the denuding effect below. During the winter, when comparatively little water was flowing, the glacier would be thickened almost everywhere at the surface, and, at the bottom, almost everywhere down to the underlying rock. Near the close of the Glacial Period a section along Windermere upper basin would, in all probability, be somewhat like Fig. 2, the ice frequently resting on the underlying rocks.

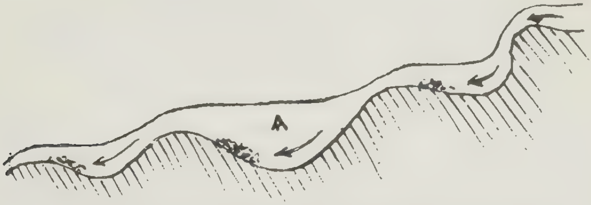

Fig. 8.—A, water, flowing mostly in course of arrows.

It is shown by Ward[1] that at the head of the lakes herein referred to and also at the head of Elter Water there are stretches of alluvial ground which he thought were originally parts of the adjacent lakes that have been filled in by detrital matter from the streams entering the lakes. If we look closely at these areas I am sure Ward's idea must be dropped. Take first the alluvial ground at the head of Wastwater. The altitude of the lake is

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[1] *Quart. Journ. Geol. Soc.*, vol. xxxi, p. 152.

204 feet. At the upper end of what Ward thought was the filled up part of it the altitude is 450 feet, or 246 feet higher than the present lake-surface. If Ward's idea is correct the outlet of the lake must be 246 feet lower than it was originally. Had that been so there would have been detrital deposits on the lake shore where Nether Beck and Over Beck enter the lake. The low ground south-west of the lake would also be more or less covered by debris from Glade How. The filled up area in Great Langdale has an altitude of about 187 feet at the lower end—Elter Water—and 500 feet at the upper end. The same objection applies to the filled in area north of Grasmere Lake. The altitude of that area at the north end is about 490 feet and at the south end only 208 feet. The altitude of the Rydal end of the alluvium extending from Rydal to Windermere is 180 feet, whilst the present surface of Windermere is only 134 feet. These alluvial accumulations, which Ward thought were filled in parts of the original lakes, were more probably produced during flood periods when the flow of mountain water was arrested to a great extent on reaching the much less inclined ground at the bottom of the valleys.

If Figs. 2, 5, and 7 be examined it will be seen that the grinding out of the lake-basins has not invariably taken place where the glaciers were thickest at the commencement of the rubbing, nor where the rocks were most likely to yield, but simply where the valleys, at the beginning of glaciation, were longitudinally not far from level. The rocks occurring on the lines of Figs. 5 and 7 are named on those drawings. Windermere is partly on the Bannisdale rocks, partly on the Coniston limestone, grits, and flags, and partly on the Borrowdale rocks. Esthwaite is partly on Coniston Flags but mainly on the Bannisdale rocks. Coniston Lake is mainly on the Coniston grits and flags, and the lower end is on the Bannisdale series. Grasmere and Rydal Water are underlain by the Borrowdale Volcanics. There is not anything at any of the lake-sites, except the nearly level valley bottoms—longitudinally—and the converging valleys at their upper end, likely to aid in the production of lake-basins.

There is nothing in the nature of glaciation, or in the character of the rocks travelled over, to produce a double basin like that at Windermere, or that at Ullswater, if only one glacier move over each lake as suggested by Ward for Windermere. That glaciers were necessary to the production of lake-basins and tarns is, I think, quite true; first to produce an immense volume of water on the surface of the ice at levels varying from say 200 to 800 feet above the lake-sites on to which a large part of it, containing rock-fragments, afterwards fell; secondly because that part of the ablation water and its included rock-fragments that did not fall directly on to the lake-sites would, on reaching the underlying strata, run towards the valley bottoms and flow in greater or less proportion over the lake-sites, according to the relative drainage areas ; thirdly, by the glaciers increasing in thickness downward, as the gouging proceeded, they would keep down the depth of the fallen water to river-form as shown by Fig. 9. These three essentials could not exist without glaciers, but the formation of rock-basins was mostly the work of ablation water charged with rock-fragments of various sizes, which had fallen down the crevasses and moulins, and were afterwards rushed along the stratified rocks lying within the areas of the future lake-basins. The Roches Moutonnées on the up-lake side of the islands of Rough Holm, Hen Holm, Lady Holm, and Belle Isle near the middle of Windermere are, I think, illustrations of what occurred at many parts of the glacier-bed, but they do not suggest the gouging out of rock-basins. The surface generally no doubt has changed somewhat since the days of glaciation but not much, in some parts scarcely any, if we may judge from the small impression made on the ice-scratches.

In my early days I did a great deal of bottom-fishing in Windermere, and was frequently struck by the irregularities in the depth of the water even in short distances, the uneven bottom being quite unlike the work of glacier grinding. Many of the irregularities resemble huge giant kettles. If, in sounding the depths of the water, the measurements were made more frequently than is usually done and in straight lines across the lake, these irregularities would be found. In looking at Figs. 4 and 5 we cannot see anything suggestive of lake-forming by ice-friction. At the beginning of the ice period the surface across the lake-areas would presumably be nearly level and the thickness of the ice afterwards would in any section be practically the same across the valley bottom. There would be little difference, if any, in the velocity of the ice at any point across the lake-area on any line of section, so far from the edge of the glacier, and therefore there would not be any difference in the friction at the side of the lake-area or at the centre. The same argument may be applied to Coniston Lake, Windermere and the other lakes under consideration.

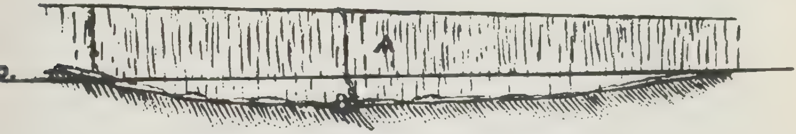

Fig. 9.—A, glacier ; B, river, produced by ablation water.

The bottom of Windermere, at its deepest, near Wray Castle, is about 100 feet below sea-level, and near the centre of Wastwater the bottom is 47 feet below sea-level. These facts were held by Ward and others to prove that rock-basins must have been made by ice. There is apparently an assumption in that conclusion that the relative level of land and sea was the same then as now. We have not any evidence. But if the assumption be correct I do not think it would prevent the large volume of ablation water held down as a stream by the overlying ice from making its way sea-ward at a great velocity and affecting considerable abrasion on the underlying rocks, particularly near the upper end of the basins where its velocity would be greatest.

The deepest part of Windermere is about 234 feet, of Grasmere 73 feet, of Coniston 174 feet, of Esthwaite about 50 feet, and of Wastwater 251 feet. When we see what has been done by Selcoth Burn we cannot think that these depths are at all great under the greatly magnified conditions of operation then existing.

Orders of Validity in Geological Theory.

By EDWARD GREENLY.

> " O monstrous: but one halfpennyworth of bread
> to this intolerable deal of sack."—I *King Henry IV,* ii, 4.

ALL of us have read how, in the infancy of modern geology, men were exceeding liberal of theorizing; indeed, it might almost be said that their theorizing, and their confidence in it, were in inverse proportion to their knowledge. Then arose the Geological Society of London, with its appeal for " new facts " and for " investigating the mineral structure of the Earth ", in other words, for application of the inductive method, and for placing geological inductions upon as broad a basis as possible. The result of which we know; the science went forward from that hour.

Most of us, probably, take for granted that the old spirit has been completely exorcised, and driven out into waste places, never to return again. Has it? The question may be worth asking, for we are apt to forget that there is such a possibility as re-action. Perhaps the best way of approach will be to attempt a rough classification of different types of geological theory, according to the kinds of induction which have led up to them, which will enable us to form an estimate of their several orders of validity.

For there are inductions and inductions. I, and certain friends of mine, have seen 20,000 rooks, and they were all black. We infer that all rooks are black. Then we meet with one albino. Patently, our induction was unsound; so we proceed to make extensive inquiries about albinos, and finding that exceedingly few have ever been recorded, we infer that the great majority of rooks are black. Not having records of all rooks, we have to admit that our induction is not absolutely perfect. But its basis is so broad that the chances of the contrary are negligible.

Now the great body of geological science is built up of inductions of this kind. They are not absolutely perfect, because it is not possible to examine *every* case of each class; but the bases of induction being exceedingly broad, the chances of their contraries are negligible. Such are our ideas as to the origin of sedimentary rocks, the consolidation of igneous rocks from a condition of fusion, the chronology of the stratigraphical systems, and so on. Their stability may be compared to that of a very stout cylinder standing on one of its bases. Let us designate them by the symbol V_3.

There is, however, another class of induction. Suppose that I has seen one rook and that a white one, and that I infer therefrom that the great majority of rooks are white, it is to be feared that my inference would be treated with levity. But suppose that someone else has seen one rook and that a black one, and infers that the great majority of rooks are black, what are we to say ? We happen to know that he is right. But his ground for a right inference is no better than mine for a wrong one. Now there is a class of problem in geology, with regard to which we have, as it were, seen only one rook. Among such may be named those which are concerned with the Endosphere of the earth,[1] in regard to which we have, as it were, seen only one rook. The endosphere is enormous, it is inaccessible, and information concerning it is exceedingly scanty. In reasoning from that information we are proceeding upon our knowledge of "one rook", and how many others there may be we do not know. The principle applicable in such cases is that which is known in logic as the Plurality of Causes combined with that of the Homogeneous Intermixture of Effects. Where we know all the antecedents, we can employ what is called by Mill the Method of Residues.[2] But what if we do not know all the antecedents ? The stability of theories on such problems may be compared to that of an exceedingly tall and slender cylinder standing on a very narrow base. They are of quite a different order of validity from those which make up the great body of the science. Let us designate them by the symbol V_1.

We may also recognize an intermediate class, concerning which we have a good deal of information, but far less complete than concerning the first-named class. Into this class fall certain problems of very great interest, among which may be named that of the tectonics of intensely folded regions of crystalline schists, where fossil evidence as to the order of succession is not available, and where, also, metamorphism has obscured the original nature of some of the rocks. Dependent upon which, moreover, is the problem as to the manner in which dynamic metamorphism has operated. Just such a region is the Mona Complex. Our theories on such subjects, accordingly, have but a moderate degree of stability, like that of a somewhat, though not excessively, slender cylinder. Let us designate this class by the symbol V_2.

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[1] I venture to suggest this as a convenient term for the interior of the Earth, within that shell for which we already use the term Lithosphere. We should then have Endosphere and Ectosphere, the latter being divisible into Lithosphere, Hydrosphere, and Atmosphere.

[2] If I may venture to offer a suggestion, it would be that none of us would be the worse for mastering Mill's classic work called *A System of Logic* ; or if that be a little too voluminous, the summary of parts of it which is given in Jevons' *Elementary Lessons in Logic*. But Mill's original is to be recommended.

Of course, this classification into a V_1, V_2, and V_3 is but a very rough one. The three classes are not sharply delimited from each other, nor are the different subjects within each class of the same order of validity. Moreover a transference is continually going on of one subject after another from V_2 to V_3. We may also hope for a transference from V_1 to V_2, though this is likely to be a slower process. The classification has been suggested merely to facilitate a sober estimate, by each of us, of the possibilities of our own subject. If, for example, we be engaged upon a V_2 problem, let us not imagine that our conclusions can be of a V_3 order. Looking back, we can easily see that our predecessors often imagined prematurely that they were in a position to deal with certain questions which were, at that time, in V_2 or even V_1, as if they were in V_3.

Certainly, any problem whatever is a legitimate subject for inquiry. But we may effect an economy of mental energy, of time, sometimes even of type and paper, by paying a little attention to the necessary laws or conditions of thought.

The Term Gotlandian and its Implications.

By Edward Greenly.

SOME of us are old enough for our geological infancy to have been nourished on books which were written at a time when the "Silurian System" was threatening to annex the whole of the Lower Palaeozoic Rocks. Most of us welcomed Lapworth's arrangement, which now seems to have won general approbation. In the interim, however, the following compromises were proposed:

	Renevier.		*De Lapparent.*
Silurique	Silurian.	Silurian	Gotlandian.
	Ordovician.		Ordovician.
	Cambrian.		Cambrian.

Neither of them has been widely adopted. It is to be noted that De Lapparent's proposal of "Gotlandian" was made because he had already made use of "Silurian" as an inclusive designation. His selection of the term was of course founded upon the fact that the beds in question are finely developed in Gothland. In a recent very able and useful work, however, "Gotlandian" reappears, but without "Silurian" as an inclusive designation, and thus without De Lapparent's reason for proposing it. What, then, is its *raison d'être*? Poetic justice, realized in the total extinction of a term which once attempted to swallow-up its compeers? That, if satisfactory to a certain kind of sentiment, would hardly be science, "Gotlandian", thus employed, is a mere substitute for the "Silurian" of Lapworth, so that the only scientific *raison d'être* would appear to be that the developments in Gothland are more typical than those in Siluria.

Suppose, then, that we apply that criterion with consistency, and see where it lands us. The *Olenellus* fauna has not yet been found in Wales, whose development cannot, therefore, be regarded as typical, so *Cambrian* must be discarded. *Ordovician* has already come under criticism in the United States, so that too is in jeopardy. Passing on to *Devonian*, who will claim Devonshire as the type-region of that system ? The *Carboniferous* does not always contain coal, and innovating America has been dealing very drastically with this term. The *Permian* system has always been somewhat unstable, " Dyas " was once proposed as a substitute, while with the great developments of " Permo-Carboniferous " which are now recognized, its future seems uncertain. As for *Trias*, that term hardly finds a defender, in regard to its scientific merits. *Jurassic* seems in better case, though it might perhaps be urged that so rapidly folded a region as the Jura mountains is not the best from which to name a typical development. *Cretaceous* is open to the same criticism as *Carboniferous*, for chalk is by no means omnipresent in the system. Cainozoic terminology stands on a far firmer footing, being based on palaeontological considerations, but if we are to name systems from regions of typical development every one of its terms must be swept away. In short, if that criterion be adopted, it is doubtful whether a single one of our existing system-names will be able to survive.

Has anyone the courage of consistency, with that as a result ? And if not, then in the case of " Gotlandian ", what do we gain ? In scientific lucidity, nothing at all. And, from the literary point of view, a ponderous word with two pairs of double consonants, instead of the euphony of " Silurian ". For such a term, is it worth while to break with the history of Palaeozoic geology ?

The truth is that our chronological terminology, though we understand one another well enough by means of it, is not an ideal one. Logicians, indeed, have made it the subject of remarks which are the reverse of complimentary, contrasting it with the nomenclature of carbon-chemistry. We live in the infancy of the science. Is it worth while to patch up this terminology ? For, some day, our successors are likely enough to " reform it altogether ". Perhaps they will apply a strictly palaeontological criterion, such as that of our own zonal sub-divisions, or that of which we have a foretaste in the names of the Cainozoic systems. Perhaps they will be able to apply criteria whereof we, with our imperfect knowledge, cannot so much as dream.

REVIEWS.

Introduction à la Géologie. By John Leuba. pp. 216, with 60 figures. Paris : Armand Colin, 1925.

It is very difficult to know how to take this little book : it is supposed to be, as the title states, an introduction to its subject, and presumably intended for readers having little or no previous acquaintance with it. But in parts, at any rate, it appears to demand for its full appreciation a very recondite knowledge of many of the most recent advances in geology. It is to be feared that an English reader with no geological education would finish it with a sense of bewilderment as to what it was all about. The treatment is necessarily extremely condensed, and some statements therefore appear to be very dogmatic and little supported by evidence : this, however, mainly arises from the omission of the necessary elementary explanations ; in a certain number of cases the explanations, when given, are definitely incorrect, in others, misleading. On the other hand, the author occasionally does good work, as when, on p. 61, he slashes at the unnecessary and futile multiplication of genera and species, as obscuring the real tendency of evolution in the organic world. On the whole, the treatment of earth-movements, folding and mountain building is good, but we cannot refrain from mentioning a truly remarkable footnote on p. 49, when epeirogenic is said to be derived from ἐπι and ὄρος, meaning movements grafted on to orogenic uplifts !

This book will have a distinct use as putting before English readers in a convenient and condensed form the trend of recent French thought in geology, but it cannot in any sense be considered as an introduction.

TRAITÉ DE GÉOGRAPHIE PHYSIQUE. By EMM. DE MARTONNE. 4th edition. Vols. I and II. pp. 1057, with 52 plates, 2 coloured maps, and 400 text-figures. Vol. III in the press. Paris : Armand Colin, 1925, 1926.

DE MARTONNE'S Physical Geography needs no introduction. It was first published in 1909, and at once took its place as one of the most complete and attractive works upon the subject in any language. It was then a single volume of about 900 pages. Later editions have increased in size, and the fourth, which is now in course of publication, will consist of three volumes. The first two of these have now appeared, and correspond with about 700 pages of the original edition. The increase is about 50 per cent.

The first volume deals with climate and hydrography, the second with the relief of the land, and the third will be devoted to biogeography. It is the second volume that concerns the geologist most directly, though there are parts of the first also which are of interest from the geological point of view. The whole has been largely re-written and re-arranged, but it retains all the qualities that led to the success of the original work. It is the clearness of the author's style and arrangement that makes the book so easy and interesting to read, and it is the numerous descriptions of actual examples that make it so useful for reference. These examples are usually illustrated by explanatory maps and diagrams, and in many cases by photographs which are admirably reproduced in the plates. Each chapter closes with a list of references.

In the section on deserts there is no notice of the observations of T. O. Bosworth in North-Western Peru, the most interesting work on desert action that has appeared in recent years; but this is the only serious omission that we have found. Amongst quite minor matters it may be noted that the author adds a "b" to the Welsh word "cwm". No doubt he has confused it with the English "combe".

THE ROCKS OF WEST LOTHIAN. An Account of the Geological and Mining History of the West Lothian District. By HENRY M. CADELL, D.L., B.Sc., F.R.S.E., M.Inst.M.E. Edinburgh: Oliver and Boyd, 1925.

THE object of this interesting volume is to present in a popular and connected manner the geological and geographical features of the mining industry in West Lothian. In the main, the information is derived from the Geological Survey maps and memoirs, but the essential features of many recent papers by the writer and others are included, so that the book may be taken as containing a summary of the latest important information on the geology of West Lothian. There are, moreover, records of private bores and other stratigraphic and structural data acquired in exploration for coal which must be of considerable interest to those studying special areas such as the Bo'ness coalfield, etc. A geological map is given at the end of the book, but this differs little from the Geological Survey publication.

After indicating the general structural features of the district, a brief discussion is given of the origin of the various kinds of coal, ironstone, and oil shale. The autochthonous theory is favoured, and in the case of a coal seam at Bo'ness by counting the number of tree stumps met in the roof of a roadway it is deduced that there must have been 395 trees per acre.

The stratigraphy of the Oil Shale Group is discussed and the shore section from Hound Point westwards which gives exposures of the rocks up to Millstone Grit age is described in considerable detail. This section of the strata also contains quartz-dolerite and teschenite sills.

Passing to the Carboniferous Limestone strata, the author emphasizes the simplicity of their structure relative to the more folded Oil Shale Group. The beds dip uniformly westwards, but their relations are complicated to a certain extent by a now buried volcano which was active up till Orchard Limestone times. This vent lay about 2 to 3 miles to the west of the site of Linlithgow, and never appears to have been much above sea-level. Caught up in the ash and lava are numerous tree stems and there are also associated chert and lime bands which are conceived to be of geyser origin. Local coal basins of very limited extent have also been formed on the flows. As well as the products of this volcano there are also sills and dykes. Of these, a basalt sill in the Bo'ness workings is described in some detail. Usually from 15 to 35 feet in thickness, it swells out locally into a laccolith 112 feet thick. The beds have been merely forced apart and the temperature of the magma has been so low that a bed of carbonaceous shale 2·5 feet thick is almost unaltered at the contact. Alteration has taken place in the case of a coal seam and here comparison of analyses shows that there has been a transference of lime and iron from the sill into the seam which must have lost hydrocarbons.

The Bo'ness coalfield is described in great detail and an attempt is made to compare the coals here with those of Fife. The Easter Main Coal is correlated with the Dunfermline Splint and the less valuable Wandering Coal with the Lochgelly Splint. The Wester Main Coal corresponds to the Jewel and Diamond seams.

The account of the solid geology of the district closes with a summary of the relations of the post-Carboniferous movements and igneous activity. It is pointed out that in the Oil Shale Group we have quartz-dolerite and teschenite sills which are cut by the faults while, elsewhere, we get both these rock types later than the movements. In these intrusions younger than the faulting, the quartz-dolerite cuts the teschenite. It is the author's opinion that the Permo-Carboniferous age of the east-west dykes is not yet fully established.

The remainder of the book describes the Tertiary history of the area and also its economic development from the earliest times. The closing chapter deals with the development of the Hilderston silver mine, now abandoned. The ore here consisted of native silver and argentiferous galena with niccolite in a gangue of barytes and calcite in a fissure near a basalt dyke.

T. C. Phemister.

Tramps Across Watersheds. By A. S. Alexander, M.A., F.G.S.E. Glasgow: John Smith & Son, Ltd., 1925.

THIS well-illustrated book is a record of a series of excursions across some of the more important watersheds of Scotland. Geology enters only in so far as it contributes to the general appreciation of the scenery of the country traversed and no new data or principles are advanced.

T. C. Phemister.

Palaeontology, Invertebrate. By H. Woods. Sixth edition. pp. 423, with 198 figures. Cambridge: University Press, 1926. Price 10*s.* 6*d.*

THE fact that this excellent book has reached a sixth edition shows that its usefulness is still fully appreciated. There are no changes in any essential features, but some new figures have been added and the whole has been revised and brought up to date, in conformity with recent developments in special branches of palaeontology. We are glad to note that it has been found possible to reduce the price, although there are several more pages and no change has been made in the general get-up. It is a cheap book as prices go nowadays, and is indispensable to all students of geology, owing to its clearness and the simplicity of its arrangement.

THE ORIGIN OF CONTINENTS AND OCEANS. By ALFRED WEGENER, translated from the third German edition by J. G. A. SKERL, with an Introduction by JOHN W. EVANS. pp. xx + 212, with 44 illustrations. London: Methuen and Co.

WEGENER'S "displacement theory", to use his own expression, is so interesting and has been so widely discussed that an English translation of the original work cannot fail to find a welcome. In this volume the translation has been made from the third German edition by Mr. Skerl and has been revised by Dr. J. W. Evans and the author himself. It is easy to read and faithfully follows the German text. The printing and paper are far superior to those of the German publication, and while the latter is almost repellent in form, the English edition is quite an attractive volume.

It is unnecessary to give any outline of a theory which is now so widely known. It deserves the most careful consideration. The great defect of Wegener's book is not that it puts forward any fanciful or far-fetched theory, but that it is entirely one-sided. So far as his own theory is concerned Wegener is quite devoid of the critical faculty, and the reader who trusts to his exposition will be continually misled as to the value of the evidence. A single instance must suffice as an illustration.

Wegener's idea that the continents are sheets of sial floating in a sea of sima is founded primarily upon the two maxima shown by the frequency curve of elevation. It is essential for his argument that there should be only two maxima. The frequency diagram which Wegener reproduces is copied from Trabert and professes to give the percentages of the earth's surface occupied by each contour-interval of 100 metres. According to the estimates of Sir John Murray, 118,000,000 square miles of the earth's surface lie beneath the sea at a greater depth than 1,000 fathoms. He had large maps of the oceans prepared upon which all recorded soundings exceeding this depth were plotted, and these maps were afterwards kept up to date for him by Dr. Bartholomew. Occasionally two or three soundings lay so close to one another that they could not all be shown, but such cases are not usual. In 1912 the total number of soundings upon these maps was 5,969.[1] Allowing for those that are missed, 10,000 is an excessively liberal estimate of the number of soundings recorded at that date which exceeded 1,000 fathoms. Trabert's diagram was published in 1911, and it is on the strength of less than 10,000 soundings over an area of 118,000,000 square miles that the lower half of the curve has been drawn. Considering the very small proportion of the earth that has been contoured, even the upper part of the curve is sufficiently pretentious, but in this lower half the pretence becomes farcical. It is a common belief among scientific men that they are much stricter than ordinary folk in their examination of evidence. Yet there are scientific men who have been convinced by this curve ; there are no sane business men who would be led into any serious enterprise by evidence so weak. No counsel would be in the least perturbed by such evidence on the other side, and no ordinary man would imagine that a single spot-height in an area of 11,800 square miles would give him any idea of its general features. The results obtained by the rapid echo-sounding methods of the present day seem to indicate that the ordinary man is right.

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[1] Murray & Hjort, *The Depths of the Ocean*, p. 131.

Nevertheless the book is very interesting. It would be still more so if the whole body of evidence were critically and dispassionately discussed.

P. L.

An Introduction to Earth History. By H. W. Shimer. pp. viii and 411. Boston : Ginn and Company, 1925.

PROFESSOR SHIMER has contributed a general text-book of geology to a field of study in which there is already a plethora of excellent elementary text-books of the conventional sort. The new book cannot be considered as following the mould ; and thus it contributes refreshing relief to the otherwise stereotyped review of geological processes followed by a chronological review of geological periods.

The book differs in two general ways from the usual text. First, material usually not included in geology is added, broadening the field ; and secondly, the emphasis is shifted in several places with considerable daring both with respect to student ability and the traditional point of view.

To illustrate the first, one may cite the inclusion of eight pages reviewing some of the elements of climatology and meteorology ; the first chapter on " Matter : its Unity and Evolution ", and the second chapter on " The Place of the Earth in the Cosmos ", together with the appended paragraphs " The Spectroscope and its Uses ". The inclusion of such material emphasizes the fact that the book is intended to develop a point of view and breadth of interest on the part of elementary students—for whom, it must be remembered, the book is solely designed. Obviously it is not intended to prepare technical students for advanced courses.

The chief criticism that may be made of the first courses in many departments in many universities and colleges is that they are frequently taught, more or less unconsciously, to prepare a small number of students for the higher courses in the department, without regard for the creation of point of view, and the stimulation of healthy curiosity and interest in environment on the part of the majority of students who will take no further courses in the department.

Professor Shimer's book is clearly of a different stamp. Whether or not it will offer elementary students who expect to take advanced work a reasonable background is difficult to say. However, it may be worth considering if intellectuality is not as important as technicality even in elementary courses.

To cite illustrations of the second way in which Mr. Shimer's book differs from the traditional elementary text it is only necessary to call attention to the emphasis given in the chapters on " Forces within the Earth " to the major fields of speculation in geology to-day, i.e., the origin of continents and ocean basins, isostasy, and the origin of tectonic mountains. The total number of pages given to these subjects is forty-five compared with slightly over fifty pages in which the work of all the agents of weathering and erosion are considered. In a well-known older text-book the proportional treatment of these subjects is about one to three. Many a writer has undoubtedly questioned the ability of students to absorb speculations in geotectonics. Undoubtedly many text-book writers have, for that reason, curtailed that portion of their treatises ; while on the other hand they have launched into voluble descriptions of the more readily understood physiographic processes. To the reviewer the emphasis in Professor Shimer's book is a distinctly healthier balance in relative importance.

When it comes to considering the details of the new text-book, the reviewer is inclined to be less laudatory. The tone of the book seems distinctly elementary, if anything, too elementary for college students ; but on this point use in classes will give the answer. A commendable effort has been made to condense—to cut out unnecessary elaboration. The result is illustrated by the attempt to make isostasy and local variations of gravity intelligible in a page—the student scarcely gets beyond the analogy of the floating corks. Epitomes are frequently as bewildering as great volumes, and as frequently connote an elementary treatment.

The work of streams is reasonably well abbreviated notwithstanding a misleading illustration. The point that the dominant activity of young streams is downward cutting while that of mature streams is no longer downward but sideward becomes obscure beside the diagram—" Idealized cross section of a valley during its growth from youth to old age "—in which the impression is given that the depth of the valley increases at regular intervals as the flood-plain develops in width.

The conception of the peneplain, with its three-fold significance of present, past, and past-incomplete plains of denudation, does not have as much consideration as its geological value, to say nothing of its teaching value, would seem to merit. Nor is the idea of plains of denudation extended to include the result of the interaction of all the agents of erosion, for which ultimate plain Johnson has suggested extending the term peneplain.

It may be questioned if the ideal proposed in the introduction of distinguishing carefully between fact and hypothesis is actually kept clearly before the student reader. Especially is the distinction lacking in the review of the history of life. Alternative hypotheses of evolutionary changes are conspicuously lacking in many instances. Frequently the reader is informed quite dogmatically that one group of animals evolved directly from another.

In arrangement of material there are points both to be commended and condemned. The inclusion of the work of ground-water under the work of streams, and of wind with glaciers, are matters of arrangement which may not have been wisely chosen considering the student point of view. On the other hand the separation of the account of organic evolution from that of physical evolution seems to be wise in an elementary book. It tends to emphasize by increased perspective the generality that, for its stimulus to evolve, life depends on the progressive change of the physical environment—a large lesson which many undergraduates fail to learn from the direct association of, say, Devonian life with Devonian geography.

In conclusion it should be noted that the book is quite up to date. An example, not only of its newness, but of the author's desire to increase the scope of study, is the inclusion of a note on the Jeans and Jeffreys hypothesis of the Earth's origin. In general the book seems to review the larger field of geology with what may be termed a cultural, rather than a technical purpose. Its breadth of view should prove stimulating to the students who study geology without thought of advancing farther in the field, though the condensation of the material may prove disconcerting. The emphasis is in keeping with the trend of research and presents a fair balance between internal and external forces which produce change and the evolution of the earth and its inhabitants.

A. C. SWINNERTON.

GEOLOGIE VON SIBIRIEN. By W. A. OBRUTSCHEW. Forts. d. Geol. u. Pal., Heft 15. pp. xix + 572, with 10 plates, 60 text-figs., and a map. Berlin: Borntraeger, 1926.

FOR many years past the geology of northern Asia has played an important part in generalizations as to earth-structure and the building of the Eurasian continent, as set forth by Suess and his successors; it must be confessed that many of the data on which these generalizations rested were of a somewhat vague and uncertain character. The work before us contains in a handy form a summary of the latest results obtained by Russian workers on this vast subject, and although it is obvious that no one else is in a position to criticize in detail the conclusions here set forth, it must be said that they seem on the whole to support most of the earlier ideas. Differences in minor points there must be, from the nature of the case, but most of the premises seem to be sound.

One of the most interesting sections is the brief summary in four pages or so of the fold systems, which appear to be numerous. The following divisions are recognized: Archaean, Sinian, Caledonian, Hercynian, Tienshanian, Kimmerian, Upper Jurassic, Saxonian, Alpine. It seems doubtful, on general grounds, whether a distinction can be drawn between Kimmerian, Upper Jurassic, and Saxonian: most authors now lump all these together as phases of the Kimmerian system.

This work is likely to remain for a long time the standard authority on its subject. As a minor point, it is to be feared that it may thus lead to a perpetuation of that Germanization of Russian names which is such an irritating feature of much English geological literature, with its complex and elephantine spelling of simple names. But this kind of thing has also extended to other continents, as when the harmless town of Ujiji in Africa becomes Udschidschi.

The weakest point of the whole book is the folding map of the tectonic regions of Siberia, which is very confused, showing rivers in blue, fault-lines in red, and tectonic areas by stippling in black, but it is explained that the cost of a coloured map would have been prohibitive: this is unfortunate, as a good coloured map would have been of immense value as setting forth in an easily assimilable form the gist of the vast amount of information contained in this noteworthy book, which gives evidence of a considerable activity of geological investigation even under the present deplorable political and economic conditions of the former Russian Empire

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THE GEOLOGY AND MINERAL RESOURCES OF BRITISH SOMALILAND. First Report. By R. A. FARQUHARSON. pp. 53. Sketch-map 1 : 1,000,000. London: 1924.

THIS Report is confessedly a preliminary one, additional details being promised when the various collections have been adequately examined. Notes on the occurrence of various minerals of more or less economic importance occupy a considerable proportion of the Report, and amongst these a full discussion of the conflicting views on the Dagaba Shabell petroleum. Of some interest from the purely geological standpoint is the recognition of a new Series of ancient sediments, the Inda Ad Slates, including quartzites and limestones. The author concludes that they are Palaeozoic or pre-Palaeozoic in age, adding " they may correspond to the slates of the Karagwe Series of the Transvaal ", a comment difficult of comprehension. No comparison is made with the slaty beds mentioned by Blanford from Abyssinia.

The references to former papers are incompletely given and one well-known author's name is misspelt.

J. P.

THE PHYSIOGRAPHY OF THE SHIRE VALLEY. By F. DIXEY. Nyasaland Protectorate Geological Survey. pp. 22. Physiographical sketch-map. Zomba, 1925.

IN this short paper Dr. Dixey gives the best account of the Shire section of the Trough Valley between Lake Nyasa and the Zambezi with which the reviewer is acquainted, dealing also with the economics of the district. The study is accompanied by an interesting sketch-map, whereon the faults are shown by bold lines and the recent beds by shading. These consist of the plateau soil framing the main valley, the gravels of abandoned sections of the valley itself, and the latest alluvium. The map brings out clearly the form of the trough and the two principal directions of faulting, respectively about 22° E. and W. of N. Where these intersect lie the Murchison cataracts. The western wall is well defined from Nyasa to the Kirk Mountains, the eastern from the neighbourhood of Zomba to Chiromo.

The latter, in part bounding the Upper Shire section of the Rift, and trending N.N.E., is in certain places later than the N.N.W. subsidence between Lakes Malombe and Chilwa.

A similarly orientated though less well-defined N.N.E. valley includes the upper portion of the Lujenda, Lake Chilwa, and a characteristic diverticulum of the main trough near Chiromo. This valley is shown by Studt in his map accompanying the paper on the "Geology of Katanga and Northern Rhodesia"; it is in the same direction and almost in the line of the faults of the Urema sunk-land, described by Wilson and Teale.

Dr. Dixey has made his Report as complete as possible, even touching on such questions as transport and water supply.

J. P.

REPORTS AND PROCEEDINGS.

EDINBURGH GEOLOGICAL SOCIETY.

17th February, 1926.

1. "The Intrusion of Binny Craig, West Lothian." By James Lunn, B.Sc. Illustrated by lantern slides.

The author called attention to the remarkable glacial channels which traverse the western part of the sill and which were described and figured by Charles Maclaren in 1839. Petrographically the rock is a basalt of unique character and quite unlike other basalts from the Scottish Carboniferous. For this reason it seems desirable to retain Professor W. W. Watts' original nomenclature—basalt of the "Binny Craig type". Analysis shows the rock to be the fine-grained equivalent of a normal quartz-dolerite.

2. "Chemical Analyses of Quartz-Dolerites and their Segregation Veins from Hound Point, North Queensferry, and Inverkeithing." By T. Cuthbert Day, F.C.S., F.R.S.E. Illustrated by lantern slides.

The author described an analysis of the fine grained quartz-dolerite at Hound Point. In this rock the ferro-magnesian minerals have been replaced by carbonates of lime and iron, while the felspars have remained fresh. It was pointed out that the intrusion of dolerite into the sandstones at Hound Point was accomplished in two stages.

Chemical analyses and microsections of the quartz-dolerites and segregation veins at Hound Point, Ferrytoll Quarry at North Queensferry, and Prestonhill Quarry, near Inverkeithing, were shown and discussed. The analyses, in each case, were made: (1) of the quartz-dolerite of the quarry, (2) of the quartz-dolerite within one inch of the segregation vein, and (3) of the segregation vein itself.

3. "The Problem of the Quartz-Dolerites." By Dr. Frederick Walker, F.R.S.E., and Miss M. F. Alexander, M.A., University of St. Andrews. Illustrated by lantern slides.

Chemical analyses make it possible to distinguish between quartz-dolerites of Lower Devonian, Permo-Carboniferous, and Tertiary ages. They also provide valuable clues as to the mode of origin of all three suites, although the exact age and affinities of the second-named suite still remain unsettled. It is hoped by further detailed chemical and petrographical investigation to throw more light on these points, as the field evidence is apparently unsatisfactory.

4. "Bombs of Nepheline-Basanite in the Partan Craig Vent, East Lothian." By T. Cuthbert Day, F.C.S., F.R.S.E., and E. B. Bailey, M.C., B.A., F.G.S.

In the *Trans. Geol. Soc. Edin.*, vol. xi, p. 344, it is pointed out that abundant and easily recognizable fragments of a greenish-grey decomposed igneous rock are restricted to the Partan Craig Vent. Microscopic examination proves the rock to be a nepheline-basanite that has undergone pneumatolysis through the action of comparatively dry carbon dioxide. Its olivine and augite are completely replaced by carbonates, with very subordinate serpentine; while half its nepheline and most of its felspar (oligoclase, albite, and probably orthoclase) remain fresh.

5. "On a Dolerite Dyke in the Rubislaw Quarry, Aberdeen." By F. M'Leod Glennie, M.A., B.Sc., University of Aberdeen.

The dolerite is exceptionally rich in biotite and contains inclusions of granite carrying sillimanite.

OBITUARY.

Benjamin Neeve Peach.

BORN 6TH SEPTEMBER, 1842. DIED 29TH JANUARY, 1926.

(PLATE XVIII.)

Charles W. Peach, of the Coast Guard Service, was transferred in 1849 from Cornwall to Scotland. Wherever he went, this born naturalist found fossils. Thus, in the South, he furnished clues to many a stratigraphical puzzle. In the North, in 1854, he proved the Durness Limestone fossiliferous. Murchison in gratitude arranged for the education of Charles Peach's youngest son at the Royal School of Mines, from which he passed in January, 1862, on to the Geological Survey. The influence of the father's fossil find did not end here. It was Charles Peach's fossils from Durness that set in train the North-West Highland controversy, in the closing of which some thirty years later B. N. Peach took a foremost place —to the lasting honour of himself and of British Science.

Young Peach's first official task was to assist Salter in the determination of fossils in the London office. In later years he often returned to this side of geology, and his descriptions of arthropods of diverse kinds, from highly specialized eoscorpions to shrimp-like schizopods, would in themselves suffice to establish a reputation; but, pre-eminently, Peach was a field geologist, and in this capacity he very soon came back to Scotland, where he was set to map glacial deposits.

At the start, it was the field, and not the geology, that attracted Peach; but soon the delight of discovery caught hold of him as he traced Highland erratics across the Lammermuirs. He had won his spurs, and from this time onwards was regarded as a worthy junior in the small group of distinguished geologists to whom we owe the wonderful primary survey of the rocks and glaciation of the Central Valley.

In 1867, Peach was entrusted with the training of a new recruit, John Horne. Thus began a partnership that lasted almost three score years, and has been the most beautiful and the most fruitful in the history of our Science—and of such a breadth that it allowed of differences of opinion on matters of first-class importance.

A few outstanding successes won by these Investigator Twins, as Heim has called them, may be enumerated, along with dates of publication: Caithness, Orkney, and Shetland glaciated from the South-East (1879–81); the North-West Highlands rendered, to quote Suess' word, transparent (1884, 1888, 1907); the whole Southern Uplands revised on Lapworthian lines, with discovery of radiolarian cherts, overlying Arenig lavas of hitherto unsuspected extension (1892, 1899); the Scottish Carboniferous reviewed (1903); Scottish lakes and rivers referred to their origins (1910).

The North-West Highlands supplied the greatest triumph. Thrusts were already in the air, when, in 1883, Peach and Horne began their work. In that year Callaway published an account of an "overthrow" locally "more than a mile in width", and Lapworth, whose health broke down in the glorious excitement, produced his "Secret of the Highlands". Still, plenty remained to satisfy the most exacting claims as regards originality. The result has been a rendering worthy of the subject. Pilgrims from all countries visit the famous Moine and Glen Coul Thrusts, and they take Peach and Horne's descriptions as their guide. In passing, it may be pointed out that the revelations of the eighties at last

Photo, S. H. Reynolds.

DR. B. N. PEACH (RIGHT) AND DR. J. HORNE (LEFT) IN THE NORTH-WEST HIGHLANDS 1912

[To face p. 188.

reconciled the *superposition* of Murchison (and Geikie) with the *dislocation* of Nicol. The latter's own opinion had been that "a comparatively very small amount of inversion and extension of older crystalline masses will suffice to explain any of the Scottish sections".

Peach was closely associated with another great Scottish geologist in the person of Archibald Geikie, who chose him as friend and counsellor in Caithness, Orkney, and Shetland, North and South Wales, and the North of Ireland. These trips were very much to Peach's heart, for they introduced him to new problems and troubled him little with cares of publication.

In addition, Peach was the companion of all; and his wide knowledge and lively imagination were pooled in the common interest. His generosity and hospitality in science and at home were proverbial.

Of noteworthy contributions to Scottish geology, not already mentioned, we may cite: The Arran vent with its foundered Mesozoic masses (1901); the twin vents of Arthur's Seat (1911); the Lower Palaeozoic age of the Highland Border rocks (*see* G. Barrow, 1901; R. Campbell, 1911, 1913); the pre-movement hornfels round the Inchbae granite-gneiss (1901, *see* C. B. Crampton, 1912); the Tayvallich schistose volcanics (1904, 1911); the glacial drainage channels of the East Highlands (1905, 1923). Other suggestions that are still under trial include: The Torridonian age of the Moines; the Lewisian inliers in the Moines; the post-Cambrian date of the Highland metamorphism.

It is essential to realize how often Peach owed his inspiration to his skill in the finding and interpretation of fossils. This vitalized his general stratigraphical outlook, and came to the front more especially in the discovery of Southern Upland radiolaria, Arran Mesozoics, and Highland Border radiolaria, brachiopods, and crustacea.

Peach was a man of striking personality. Again and again he has been compared with a lion or an eagle. This was in part due to his handsome head and his great vigour and personal strength, and in part to his searching glance that seemed to penetrate at once to the heart of a problem and to suggest an immediate line of attack. In conversation, he was a master. He would defend a position with an emphasis that was apt, indeed, to be mistaken for dogmatism; but at the same time, there was no one more ready to reconsider a cherished conviction in the light of new evidence. Extraordinary as it may seem, considering his dominating position in Scottish geological thought, he trusted almost wholly to conversation for his intercourse with the scientific world. He could scarcely bring himself to write or to read. It is curious too that the power and confidence, so abundantly expressed in conversation, forsook him when he was called upon to speak in public—except in the open, where he excelled as leader of excursions, and also on the happy occasion of his presidency over Section C at the Dundee meeting of the B.A., 1912, when he surprised and delighted all his friends. Peach was himself, too, whenever he handled a pencil, and will long be remembered for graphic sections across mountain chains; portraits of fossils, and maps, alike beautiful and true.

It is characteristic of his great boyish spirit that his thoughts never rested on achievement, but were ever concentrated on problems of the present and the future, some of them important, some trivial. He received the Wollaston Fund, Murchison Medal, and Wollaston Medal, as well as the 1892 Murchison Centenary Prize, from the Geological Society of London, and the Neill Medal from the Royal Society of Edinburgh. He was elected F.R.S. in 1892, and made LL.D. of Edinburgh University in 1903.

E. B. Bailey.

CORRESPONDENCE.

THE SUDBURY NICKEL ORES.

Sir,—Professor Coleman's invaluable map and memoir of the Sudbury district have been of such great service to all students of that instructive mining field that his restatement of the hypothesis that its ores were formed by segregation in the molten norite—published in the last number of this Journal (1926, pp. 108–112) in criticism of my brief summary of the arguments against that view ("The Physical Chemistry of Igneous Rock Formation", *Trans. Faraday Soc.*, No. 60, vol. xx, part 3, 1925, pp. 454–6)—will carry great weight. He supports the formation of the ores by igneous segregation on the following grounds.

(1) That those who have mapped the area unanimously adopt that theory. So far as I know the literature the hypothesis has been rejected in recent years by a great majority of those who have done extensive field work in the locality.

(2) The main support is now attached to the pyrrhotite in the norite which Professor Coleman claims can only be explained as a magmatic segregation. As evidence he instances the Frood Mine of which the petrography has been described by Dr. Howe (*Econ. Geol.*, vol. ix, 1914, pp. 508–14) and the structure by Miller and Knight in their Report to the Ontario Nickel Commission (1917, pp. 196–201 and 218). These and other accounts show that so far from the pyrrhotite there being a primary constituent of norite, it occurs as a secondary material in a belt of fractured rocks of various kinds. According to Howe the ore is partly mineralized diorite and partly mineralized gabbro; Miller and Knight add that it is partly also mineralized quartzite and graywacke. The country rock at the Frood Mine has been fractured and the sulphides act as a cement to the fragments, both in the gabbro and diorite, though in the latter the sulphides are also

present in blebs. The rock of the ore body has been crushed, brecciated, and sheared, and Miller and Knight state that this fractured zone was an ideal channel for the circulation of mineral-bearing solutions. They give a figure (op. cit., p. 199) of Frood ore to show that "the sulphides replace or impregnate the rock". So far from the ferro-magnesian minerals in the igneous rocks being unaltered, Howe figures Frood ore and shows the sulphides and quartz replacing hornblende, and I have seen many slides from other mines in which the sulphides replace biotite and hornblende and the hypersthene is much altered. The evidence from the Frood Mine, both from the field occurrence and the microscopic character of the materials, seems absolutely inconsistent with the formation of the ore as a magmatic segregation in an igneous rock. The facts indicate the ore-formation by mineral-bearing solutions impregnating a crushed zone of different types of rock. Professor Coleman quotes Dr. Howe in support of his own view; but Dr. Howe states (*Econ. Geol.*, vol. ix, 1914, p. 512) that the Frood rock has been dynamically metamorphosed, and (ibid., p. 508) that "it offers serious objections to the confirmation of the view held in the field that the sulphides in the Frood rock were of magmatic origin". Dr. Howe, it is true, considers that the nickel may have been an original constituent in the rock; but he points out that even so its relations to the silicates have been so changed by the metamorphism that there is no conclusive evidence of its magmatic origin.

(3) In reference to Dr. Harker's rejection of the view that the "norite" and the overlying micropegmatite are derived from the same magma, the norite being due to gravitational segregation of the heavier constituents, Professor Coleman is sure that his specimens would convince Dr. Harker. I also feel sure that if Dr. Harker saw the material collected by Dr. Phemister, and had seen his field evidence that the most basic part of the "norite" is near the top and not far from its sharp contact with the micropegmatite, that Dr. Harker would be fully confirmed in his conclusion that the two rocks are not due to gravitational differentiation.

(4) Great importance is attached by Professor Coleman to blebs of ore which are completely surrounded by silicates in the igneous rocks. The blebs are said to be identical with the material in the ore veins, and therefore evidence that the veins are due to magmatic segregation. These blebs of ore, however, also cocur in the quartzites, and in these rocks their origin by segregation is clearly impossible. The evidence that the veins and the ores cementing the fractured silicates are identical with the blebs may therefore be equally well cited as evidence that the blebs are not due to segregation. Sulphides completely isolated in rocks yet due to replacement are not uncommon.

(5) The greater importance attached to faults and fractures in association with the ore bodies is a welcome development. The

hydrothermalists have long called attention to the occurrence of the ore bodies in connexion with faults which explain their position.

(6) My statement that some of the nickel ores occur outside the norite is represented by Professor Coleman as founded on a statement by Mr. Knight. His statement was in my mind, with much other evidence, including the facts seen in a visit to the Victoria Mine in 1908, where the ore occurs in greenstones and not in the norite, with which there appears to be no connexion. Professor Coleman attributes the sulphide ores outside the " norite " to their injection as molten sulphide from the overlying igneous rocks ; but that explanation would appear to me quite inapplicable to the ores in the " greenstones " of the Victoria Mine.

J. W. Gregory.

Glasgow.
March, 1926.

THE SUDBURY LACCOLITE.

Sir,—Professor Coleman in his reference to me (p. 110) confuses together two questions which are, to my mind, quite distinct, viz. the relation of the norite to the underlying ore-bodies and its relation to the overlying granophyre. On the former question I am quite at one with him ; but I cannot accept the " transition " from norite to granophyre as evidence that the two have been differentiated in place from one common magna under the influence of gravity.

The thick sheet of norite, like other large bodies of basic rocks, is variable, but I can see in it nothing resembling a vertical variation in accordance with density The thick sheet of granophyre is less variable, and again without any appearance of order. Between the two comes a much smaller thickness of rocks of generally intermediate composition, not sharply divided from either norite or granophyre. These rocks vary irregularly, with a strong suggestion of heterogeneity, and closely resemble some familiar to me in the western isles of Scotland, where they are demonstrably hybrid products. The natural inference is that the granophyre was independently intruded after, but not long after, the sulphide-bearing norite, and that some commingling took place where the two came together. My own acquaintance with the Sudbury laccolite is limited to one brief visit and the study of a series of specimens collected, but all that I have since read (of observed fact as distinct from speculation) has gone to confirm the opinion then formed.

Alfred Harker.

Cambridge.
12th March, 1926.